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AN ADDRESS

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With this semi-annual meeting begins the sixth year of the existence of the Northern District Medical Society. At the commencement of the second year of its existence, its then President—Dr. David Powell, of Marysville—expressed the hope that the Society would, in time, embrace among its membership, a large proportion of the progressive physicians of the northern part of the State, from San Joaquin and Solano to Siskiyou and Del Norte. I think his hopes have been fully realized. The Society is making constant gains, and appears to have the respect and interest of a large number of the physicians in the counties from which it obtains its membership.

The benefits to be derived from membership in a medical society are underestimated by many physicians. The assemblage of medical men from different parts of the State not only tends to promote fraternal relations, but spurs one on to further study and observation. The association contributes a defense to the interests of the medical profession in matters of economy, as well as where, by its authority, it can assert itself in medical questions of interest to the community at large, especially on the subject of hygiene.

One of the chief aims of our Society should be to further scientific knowledge and investigation. In this connection, I wish to bring up the subject of the need of an efficient reference medical library on the Pacific coast. I have thought of this for some time, and was not aware, until a few days ago, that the subject had already been spoken of by a former President of the Society, Dr. W. A. Briggs, in his address at the semi-annual meeting at Woodland, two years ago. Comparatively little has been accomplished in this direction, and being a matter of great necessity to medical research and original work, the subject will bear repetition. If the State and district medical societies, assisted by every county society of the State, would labor for this end, the foundation of such a library, located at some central point—for instance, Sacramento—could probably be accomplished.

Perhaps, of the greatest value to members of medical societies is the opportunity afforded for the interchange of views, in the discussion of papers. Criticism and discussion bring out differences of opinion and counteract the *post hoc ergo propter hoc* way of reasoning, which has led to the development of fads, and which has interrupted medical progress. Credulity is, in fact, as prevalent a tendency among medical men as among laymen. As far as the latter are concerned, there is, perhaps, not a thing in existence that people can not be induced to try, if proclaimed to be a cure; homeopathy, magnetism, electric belts, etc. That many recover under the treatment, or even by the treatment, *i. e.*, by reason of the suggestion brought about by the cure, is undeniable; and it explains why people continue to have confidence in almost any kind of treatment, provided, that it is fashionable, and that it has been sufficiently advertised.

It is to be regretted that the same credulity is also found among physicians; and there are not a few, who find brilliant results from almost any new treatment, until this again is given up. This may, perhaps, to some extent, be a good thing, as the confidence may be communicated by the physician to the patient, and by reason of the suggestion, act beneficially on the latter. It does not, however, add to the glory of the former, to be giving up a treatment, after a few months or years, which numerous physicians had proclaimed a great discovery. Thus the treatment of tuberculosis by tuberculin injections, as practised a few years ago, has probably greatly lowered the esteem in which medical science has been held by the intelligent and thinking layman. Fads have, for a time, had their sway in all branches of medicine.

Confining myself to what I have experienced of this kind, in my short medical life, on the subject of nervous diseases, the first one I remember was nerve-stretching, introduced by Prof. Nussbaum, of Munich, and proclaimed to be *the* treatment for tabes. This was based on Langenbuch's case, which a *post-mortem* examination afterwards proved to be a peripheral pseudo-tabes. Wherever nerve-stretching was tried the tabetic pains were alleviated, the gait was improved, loss of sexual power restored, etc. In other nervous diseases it relieved pain now and then, removed contractures and other spasms, and in this way made a tour of conquest from Europe to America. The resulting paralysis and occasional deaths, came out later, with other unfavorable results; and where is nerve-stretching now the mode of treatment? This was followed by stretching of the spinal cord by suspension, applied in tabes and other diseases of the central organs, which received the same favor at the hands of neuropathologists, but which is now a thing of the past.

The next conqueror that appeared on the field was hypnotism, which undertook to remedy not only functional neuroses, but diseases of organs outside of the nervous system, and was even supposed to affect beneficially drug habits and to reform criminal tendencies. It was applied in tabes to relieve the lancinating pains, the incontinence of urine, etc. While hypnotism is not yet dead, its voice has almost been subdued to a whisper. In this connection one is reminded of Keeleyism and its golden discovery, the present status of which has made the adjective of "discovery" only applicable to the financial benefits which have accrued to its thrifty originator. Then came Brown-Séquard's elixir of life, which was said to be the long-sought-for fountain of youth, and which was applied in tabes, as well as in neurasthenia and hysteria, with varying success. This treatment, however fanciful as first introduced, led to the introduction of the various tissue-juices, which has, at least as far as one experience is concerned—the treatment of myxedema with the juice from the thyroid gland—given us, it would seem, a valuable remedy. At the present time we inject. We are living in the age of serum-therapy, and it is to be hoped that this is not doomed to the great nothingness to which so many "great discoveries" of the past have been consigned.

This is, perhaps, a rather pessimistic view of medical therapy, and I hope you will not conclude from my remarks that I am opposed to experimental investigations. I have simply given these illustrations in emphasis of conservatism, and the importance of careful scrutiny of what is brought to the notice of the medical profession, which I think should, as far as our Medical Society is concerned, encourage the expression of individual opinion and a thorough discussion of every subject presented.

THE DIFFERENTIAL DIAGNOSIS OF DIPHTHERIA.

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Read before the San Francisco County Medical Society.

The object of this paper is a plea for a general introduction into this city of a method of diagnosis in diphtheria which is usually, if not generally, employed abroad, and in many cities of our own country.

Cases of sore throat are constantly occurring, whose real nature, by a clinical examination alone, it is impossible to tell. In some of these, extensive pseudo-membranes exist, presenting all the clinical appearances of diphtheria, but which, in reality, are not of so serious a nature, and generally get well without leaving any traces. Other cases of sore throat, in which the local symptoms seem slight, nevertheless show their real nature by the constitutional symptoms to which they give rise, and by their sequelæ.

The importance of making an early and exact diagnosis, under such circumstances, is apparent to all. Until recent years this was impossible, and many errors in both directions were made, entailing needless hardship on the one hand, and, on the other, often allowing the rapid spread of diphtheria, by permitting infected individuals to mingle freely with healthy persons.

According to the Secretary of the Board of Health of San Francisco, the following is the number of deaths occurring annually in this city, during the last five years, from diphtheria and croup: 1891—Diphtheria, 314; croup, 143; total, 457. 1892—Diphtheria, 229; croup, 89; total, 318. 1893—Diphtheria, 153; croup, 73; total, 226. 1894—Diphtheria, 38; croup, 39; total, 77. 1895—Diphtheria, 21; croup, 27; total, 48. Making a grand total, during five years, of 1,126 deaths occurring in this city from this cause alone. These results may be taken as correct, although bacteriological examinations were not made, for statistics of fatal cases, in which such examinations have been made, invariably show the presence of the diphtheria bacillus.

The best means of making a correct diagnosis of diphtheria is by such a bacteriological examination. When, therefore, a case of membranous sore throat occurs, this should be done at once, in order to determine its nature. Before going into this part of the subject in detail, it may be well to give a brief review of the investigations which led to the discovery of the bacillus of diphtheria, and to the consequent separation of the various forms of membranous sore throat, with which it had hitherto been confounded.

From an anatomical point of view, diphtheria is an inflammation of a mucous membrane, characterized by swelling, redness, and the exudation of a false membrane. Fibrinous false membranes can even be produced experimentally, by touching the mucous membrane of an animal with ammonia, or by profound or even superficial irritation. But a false membrane, so produced, does not constitute diphtheria. This disease is more than a simple inflammation, for it attacks the ganglions, produces infection of the system, and is contagious.

The specific nature of this malady was first established by Bretonneau and Trousseau. This disease had been observed from the greatest antiquity, but its different manifestations were attributed to different diseases. Diphtheritic pharyngitis received the different denominations of "Egyptian," "Syrian," "pestiferous ulcer," "gangrenous sore throat," etc. In different epidemics, diphtheritic laryngitis was called "garotillo," "morbus strangulatorius," "suffocating sore throat," etc., but no relation was established between these different diseases.

Home, a Scotchman, in 1765, created the name of croup, and so separated the affections of the larynx from those of the pharynx. Bard, in 1771, proclaimed the identity of the different forms of diphtheria, but it was reserved for Bretonneau to establish, by clinical and pathological facts, the common origin of these different manifestations. He has also the merit of separating true croup from false croup. He gave the malady under consideration, the name of diphtheritis. Trousseau spread the doctrines of his master, Bretonneau, and changed the name of the disease from diphtheritis to diphtheria.

The ideas of Bretonneau and Trousseau were generally admitted, but met with opposition in Germany. Virchow and Rokitansky endeavored to overthrow the theory of identity formulated by Bretonneau, and tried to prove, by pathological facts, the difference between diphtheritic and croupous inflammations, but they only succeeded in reëstablishing confusion. The ideas of Bretonneau, however, prevailed in the end, and the light thrown upon this matter, by bacteriological investigation, only confirms his opinions.

As soon as investigations were made in this direction, it was found at once that an infinite variety of bacteria are present in the false membranes of diphtheria, and in the profound inflammations which they produce. Spores and vibrions were found in 1861, and later, various kinds of microörganisms. This, however, did not advance the question much.

Klebs, of Zurich, in 1873, was the first to give an exact description of the bacillus of diphtheria. His microsporon diphtheriticum was composed of rods and of micrococci. The microscopic examination of the pseudo-membranes which he made showed micrococci —isolated, in groups, or in chains—or else bacilli, isolated or in groups. These microörganisms were always most abundant in the superficial part of the pseudo-membranes.

By microscopical examinations alone, it was, however, impossible to tell whether the micrococci and bacilli belonged to one species or to several, and whether only one of them or all of them were necessary to engender the disease. In order to find this out, it was necessary to isolate each kind of microörganism, and to cultivate it separately.

Loeffler, in 1884, was the first to do this successfully. Having examined, under the microscope, many sections of mucous membranes, covered with diphtheritic exudations, Loeffler saw that these sections contained bacilli and streptococci. He was able to isolate portions of the pseudo-membranes containing only the streptococci. He made repeated cultures of them until the cultures were pure. These pure cultures of streptococci, inoculated in animals, gave rise to inflammations, at times intense, but they never produced pseudo-membranes. It therefore followed, that the micrococci, in chains, were not the cause of the diphtheria. On the other hand, this variety of micrococci does not always exist in diphtheria, and it is found in other diseases.

Loeffler could not obtain the reproduction of the rods on peptonized gelatin, but he succeeded in cultivating them on gelatinized blood serum, which he inoculated with fragments of the pseudo-membrane from the pharynx. The chains and rods developed together, but in successive cultures, the colonies of bacilli were large and opaque, while the colonies of chains were small and transparent. After this it was easy to separate one kind from the other, and to cultivate the bacilli alone.

Having obtained pure cultures of the bacilli, Loeffler began experiments on animals. He inoculated them with success, and so gave the material proof of the specificity of diphtheria. He found that if the liquid from the culture be placed on a healthy mucous membrane there is no effect; but if the mucous membrane is inflamed or abraded, then there develops in certain animals at the site of the inoculation an edema, a psuedo-membrane, or a hemorrhagic exudation. Mice and rats resist this inoculation; Guinea pigs are very sensitive, no matter where inoculated. These animals die in 2 to 8 days. Small birds and rabbits are also very susceptible. In all these animals no bacilli are found at the time of death.

The results obtained by Loeffler were confirmed by observers in different countries. Further investigation proved that the bacilli do not penetrate into the tissues. Hence it was concluded that the general symptoms must be due to penetration into the circulation of a poison produced by the action of the bacilli.

Roux and Yersin proved that the bacilli do secrete a poison, for, by injecting sterilized cultures of the bacilli into animals they produced paralyses and all the symptoms of diphtheria except the membranes. Later on they even succeeded in isolating the active poisonous principle contained in these sterilized cultures. They obtained this active principle in the form of a white or yellowish powder. They filtered pure cultures of the Klebs-Loeffler bacilli through porcelain. On the addition of alcohol to the clear, germ-free filtrate a white powder was precipitated. This was redissolved in water and again precipitated with alcohol. The powder thus obtained, when inoculated in animals, produced paralysis and all the symptoms of diphtheria, just as does the injection of the sterilized cultures.

The bacilli of diphtheria are straight or slightly curved at the end. They are as long as the tubercle bacillus, but thicker. The largest are composed of several segments. Their ends are thickened. They have no movement, and are well and readily colored by methylene blue. Sometimes the ends are more deeply colored than the center. They are destroyed at 140° F., which proves that the extremities are not spores, as spores resist a higher temperature than this.

To develop cultures of the bacilli the temperature must exceed 68° F. At 98° F. the culture remains as fertile as when fresh. The bacillus of diphtheria can be found in the discharges from the mouth and nose, and in the mucus of the pharynx and mouth, even before the development of the false membrane, and when there is as yet only a catarrhal inflammation present. Roux and Yersin have found them 14 days, and Ritter 5 weeks, after the disappearance of the false membranes. Persons who have been in contact with diphtheritics can, without being themselves sick, have in the mouth or throat virulent diphtheria bacilli, capable of transmitting the malady to others.

The histological examination of the psuedo-membrane shows a fibrinous network imprisoning an amorphous substance, mucin, leucocytes, epithelial cells and numerous micro-örganisms besides staphylococci, streptococci; more rarely diplococci, pneumococci and bacilli coli commune, and the bacilli of Loeffler, which are the only specific microbes.

While in some cases, which have been named *pure diphtheria*, no microbes are present except the Loeffler bacillus, in the majority of cases of diphtheria other microbes are found in conjunction with it. The gravity of a case of diphtheria is usually augmented by the presence of these associated microörganisms, as they play an important part in its complications. The most dangerous of these is the streptococcus. It is in cases where the streptococcus is associated with the Loeffler bacillus that we have all the evidences of sepsis, suppurating glands, broncho-pneumonia, nephritis, etc. Sometimes the staphylococcus is found in connection with the bacillus of diphtheria. Its presence also foretells suppuration, but to a less degree than is indicated by the streptococcus. Often all three forms exist together in a membranous sore throat. The association with the diplococcus or the coccus of Brisou is less unfavorable.

In addition to membranous sore throats, due to the presence of the Loeffler bacillus alone or in association with other microbes, membranous sore throats frequently occur in which the Loeffler bacillus does not exist, and which, therefore, are not diphtheritic in character. These varieties of membranous sore throat are also of microbic origin. They are caused by the streptococcus, the staphylococcus, the micrococcus of Brisou, etc., existing either alone or in association with each other.

According to Lennox Browne, the membranous sore throat occurring in the early stages of scarlet fever is due to the streptococcus alone. The membranous sore throat, which occurs later on, is apt to be an association of the Loeffler bacillus, and is then a case of true diphtheria. The membranous sore throat, due to streptococci alone, may also be found after the other exanthemata, especialy after measles.

The membranous sore throat due to the presence of staphylococci alone is more yellow in color than sore throats due to the bacilli of diphtheria, or to streptococci; and it is found in individuals exposed to unsanitary conditions, or who are "run down."

Another variety of non-diphtheric membranous sore throat is due to the small micrococcus described by L. Martin. He has named it the Brisou micrococcus, after a child who, on seven different occasions, was received into the diphtheria wards of the Hôpital des Enfants Malades, with its throat full of white membranes diagnosed as diphtheritic. On each occasion, however, bacteriological examination of the membrane showed only a small micrococcus to be present. The disease had no serious consequences.

Lennox Brown states that twelve different varieties of membranous sore throat exist. They are as follows: (1) Contains nothing but the diphtheria bacillus. (2) The diphtheria bacillus associated with streptococci. (3) Diphtheria bacilli, streptococci and staphylococci. (4) Diphtheria bacilli, streptococci and diplococci. (5) Diphtheria bacilli and diplococci. (These are all varieties of true diphtheria.) (6) The sixth variety of membranous sore throat contains streptococci only. (7) Streptococci and diplococci. (8) Staphylococci only. (9) Staphylococci and diplococci. (10) Diplococci only. (11) Diplococci and a mycelium. (12) The twelfth is indeterminate. These last seven varieties are non-diphtheritic, or pseudo-diphtheritic in character.

Since membranous sore throat may be of so many different varieties, it is not surprising that children who do not have diphtheria are too often received into diphtheria hospitals, where they run the greatest risks. Of 448 children treated by Roux in the diphtheria wards of the Hôpital des Enfants Malades, 128, more than a quarter, did not have diphtheria.

According to the thesis of Madame Bonnier, mistakes in the clinical diagnosis of diphtheria are made once in every three cases.

Since true diphtheria has so many forms of its own, and since there are other diseases with which it can be confounded, how are we to distinguish one form from the other? and how are we to recognize the benign cases from the malignant ones?

The great importance of this question from the standpoint of diagnosis, prognosis, treatment, and prophylaxis, has induced me to bring this matter up for your consideration this evening. While actual experience with cases of diphtheria may, to a certain extent, guide us in our diagnosis of the character of a membranous sore throat, still, with our present knowledge of the real cause of the disease, we cannot rely upon experience alone.

There is one method, and but one, by which we can gain exact and certain knowledge of the nature of the membranous sore throat with which we are dealing, and this is by the bacteriological examination of the false membranes themselves.

There are two ways of doing this: (1) By the direct microscopical examination of the membranes. (2) By means of cultures made from them. The direct microscopical examination is made as follows: A piece of the suspected membrane is removed from the throat by means of a sterile cotton swab. The swab is immediately placed in a sterilized test-tube, which is at once closed with a plug of cotton and protected with a rubber cap.

On reaching his laboratory the physician picks off the membrane from the swab with a pair of sterilized forceps, dries it carefully with blotting paper, and then smears it on a covered glass. It is then dried over a flame. The specimen is next colored with methylene-blue, or other appropriate coloring matter, after which the excess is washed off and the specimen again dried and examined under an immersion lens.

If the bacilli of Loeffler are present, the diagnosis of diphtheria can at once be made, and some idea may also be had of the germs associated with them. A diagnosis can be made in this way in about 50 per cent. of the cases. It is, however, not so characteristic as the method by cultures, and besides a negative result will not prove the absence of the disease, as the patient may have diphtheria and yet no bacilli be present in the portion of membrane examined. In such a case it would be necessary to resort to the method by cultures.

This method recommends itself on account of its great accuracy and its extreme simplicity. Solidified blood serum is one of the best culture media for the diphtheria bacillus. It should be the duty of Boards of Health to keep a supply of blood serum tubes on hand for the use of the profession. In the absence of this source of supply physicians ought to be able to secure prepared tubes at the drug stores. If prepared in a reliable bacteriological laboratory the tubes keep indefinitely. In all large cities, and even in many country places abroad, there is no difficulty in procuring the tubes. In New York, Boston, and other eastern cities the Boards of Health supply the serum tubes on demand. In some places a serum tube and a sterilized tube containing a swab are done up in a neat little package all ready for use.

When a physician is called to a case of suspicious sore throat all he has to do is to procure a blood serum tube and a sterile swab of cotton. On arriving at the bedside he directs the patient to open the mouth. He then depresses the tongue as in any ordinary examination of the throat, introduces the swab and gently rubs off a little of the membrane. The particle of membrane so procured is introduced into the tube, rubbed gently over the surface of the serum and then withdrawn and the plug of cotton replaced.

As soon as the physician leaves the house he sends the inoculated tube to a bacteriological laboratory. Immediately upon its receipt it is put into a thermostat at 98° F. In 14 to 18 hours the cultures are sufficiently advanced for the bacteriologist to be able to make a diagnosis. The colonies of diphtheria bacilli appear as minute whitish-grey dry points thicker at the center than at the periphery. If no such colonies appear in 24 hours the case is not one of diphtheria. The colonies of the associated microbes do not grow so fast as those of the diphtheria bacillus. When the cultures in the tubes are sufficiently advanced one of the colonies can be picked up, placed on a slide and examined microscopically.

The result of these bacteriological investigations is sent at once to the physician in charge of the case. He thus obtains a positive diagnosis of the nature of the disease within 24 hours of first seeing the patient. If no Loeffler bacilli have been found he knows that the case is not one of diphtheria, and that the quarantine of the patient can be removed at once. If, on the contrary, the Loeffler bacillus is present he increases his vigilance and treats the case accordingly.

The advantage of this method of diagnosis is its precision. It substitutes absolute knowledge for uncertainty and doubt. It enables the practitioner to know, in the course of a few hours, positively the nature of the malady with which he has to deal. Where this method has been put in practice it has been shown that only one case in four, diagnosed clinically as diphtheria, is really of this nature. When the bacilli of Loeffler have been found in the membrane, the physician, being sure of the nature of the case, can act vigorously and promptly, and so not only save many lives, but also more surely protect others and prevent the spread of the disease by strict isolation of the patient. He can further protect the community by having repeated bacteriological examinations of the secretions from the nose and throat of the patient made, and by not discharging him, even though all traces of membrane have disappeared, until the bacteriological report declares that no more bacilli can be found.

Since such a sure and simple method of diagnosis has been found, it would seem to be the duty of the profession always to employ it, not only in cases of suspected diphtheria, but in cases where there is any possibility of diphtheria being present. By early and certain detection the spread of this dreadful scourge can, to a great extent, be prevented. Why cannot the serum tubes for purposes of diagnosis come into general use here?

Note.—Since this paper was commenced the Board of Health has taken preliminary steps towards procuring the serum tubes, and making the required examination. If this plan be carried out it will then be easy for us to employ the only accurate means of diagnosing a disease which we have always among us, and which already in the history of California has claimed so many victims for its own.

814 Sutter street.

A NEW THEORY ON THE CIRCULATION OF THE BLOOD IN THE ARTERIES.

By C. H. ROSENTHAL, M.D., San Francisco, Cal.

Read before the San Francisco Medico-Chirurgical Society.

My paper this evening is one that is out of the ordinary routine of papers read before this Society, inasmuch as it is strictly scientific, and also strictly physiological. Scientific in the sense that, if the theory be true, it is a mathematical truth, a physical truth, whose application is as universal as the law of gravitation, as true as the theory that the earth revolves around the sun. It is as exact in its application as any law of mathematics or physics.

I believe that my theory as to the circulation of the blood stands in its relation to the form and mechanism of the heart, just as the theory of light stands to the eye, or the theory of sound to the ear. So far, we have no theory necessitating the existing form of the heart or aorta; and no theory explaining why the valve between auricle and ventricle should differ so entirely from that between ventricle and artery.

Our present theories of the circulation of fluids pay as little regard to the form and mechanism of the circulatory system, as does the materia medica of homeopathy to pathology, or its therapeutics to physiology. I have prepared for this evening's paper, a statement of facts to prove, first, some of the errors of our present theory; and second, to prove the truth of my own.

With this short preamble, I shall now hasten to prove that, just as the earth revolves on its axis while moving through space, just as the projectile fired from a rifled cannon rotates on its axis while moving forward, so the blood, after leaving the heart, assumes an intense rotary movement while circulating through the arteries.

As self-evident truths I may state:

1. That the blood being a liquid of high specific gravity with solid constituents held in mechanical suspension, it must be kept in constant motion to preserve its homogeneousness.

2. That in the heart the greatest pressure must not be brought to bear on the part capable of least resistance.

3. That the mechanism of the heart must work with machine-like precision—that is, an artery and its valve must of necessity maintain certain and relatively constant relation to each other as regards space and angle, which are constantly changing at the expense of time and force.

To establish the fact of the existence of a rotary motion in the circulating blood and therefore a centrifugal power, I can prove:

1. That the form of the ventricles is such, that only a spirally-moving liquid can flow through them.

2. That the form of the aorta and pulmonary artery is such, that any liquid caused to flow through them obtains a rotary movement.

3. That the mechanical action of the heart produces a rotary motion of the blood with its resultant centrifugal power.

4. That the pulse, the rythmical dilatation and contraction of a blood-vessel, as it occurs in the arteries, can only be caused by a rotary-moving fluid under pressure; inversely, this dilatation is inducive to and intensifies the rotation of a rotary-moving fluid.

5. That the aortic and pulmonary semilunar valves can be perfect valves only in an intensely rotary-moving current.

6. That, given a system of conduits as in the circulatory system, any fluid flowing rapidly through them under pressure must rotate and necessarily possess and exert a centrifugal power in direct proportion to the rotary force.

7. That all moving fluids rotate, or tend to rotate, and, therefore, possess a centrifugal power.

8. That the rotary motion and consequent centrifugal power are absolute necessities in this circulating medium to keep its integrity intact and to preserve all its normal characteristics. As soon as this motion ceases, the blood contained in the larger conduits begins to precipitate and perhaps disintegrate.

The fifth proposition that: The aortic and pulmonary valves are perfect valves only in an intensely rotary-moving current, is so simply and purely scientific, so capable of graphic and indisputable physical and mathematical demonstration, that I have selected it for my subject this evening.

The aortic and pulmonary valves are each composed of three cusps or pockets, relatively identical in form, position, and strength, so that what is said of one applies to both. I shall first take up the aortic valve.

The segments of this valve are entirely within the aorta, and self-evidently the blood must pass beyond these segments to permit them to close without any regurgitation of the blood into the ventricle. The arch of the aorta is not only curved, but twisted and bent. The sinuses of Valsalva lie on different planes; therefore these segments and their respective nodules, each being opposite a sinus, lie on different planes. The aorta is in a constant state of dilatation during life, but during systole of the ventricle the aorta is in diastole, in the act of dilating, and during diastole of the ventricle the aorta is in systole, in the act of contracting.

The ascending portion of the aorta, including the sinuses of Valsalva, represents a spiral-shaped truncated cone, or funnel, standing on its base, the sides of which are bellied out; so that, although the bottom of the funnel, the opening next to the heart, is narrower than the top, the bellied-out portion makes the body of the funnel the widest. This much is capable of anatomical demonstration, and is, therefore, incontrovertible.

Let us now take up one revolution of the heart, one cycle of the ventricle and semi-lunar valve action, from the beginning of systole to systole. As an established fact, we know that the aorta dilates almost synchronously with the contraction of the ventricle; that, coincidently the segments of the semilunar valve must be separated, because during this period of time the blood is flowing through the valvular opening; that, during systole of the aorta, the ventricle is in diastole, and as no blood regurgitates from the artery through the valvular opening into the ventricle, the valve must have closed in time to prevent it. The systole of the ventricle takes $\frac{9}{80}$ of a second in the adult human heart; the diastole $\frac{16}{30}$; altogether $\frac{25}{30}$ or $\frac{5}{6}$ of a second.

We know that when the ventricle contracts the aorta lengthens as well as dilates, also that the heart contracts simultaneously from base to apex, and from apex to base. I say simultaneously—I mean to correct that afterwards. Other established facts are that the valve closes coincidently with the reception of the ventricular contents within the aorta, and that the ventricle remains in systole about $\frac{8}{30}$ of a second *after* the valve has closed, or after the blood has left the ventricle.

I shall now take up the straight-moving current, or present theory, and see how it agrees with or fits into these well-known, easily demonstrated and incontrovertible scientific facts. As we proceed I shall point out the disparity between theory and fact.

The systole of the ventricle occupies $\frac{3}{10}$ of a second; this time has been divided into three periods, each $\frac{1}{10}$ of a second long. The first $\frac{1}{10}$, in which the systole exists, before the flow of blood is established from the ventricle into the artery, is taken up to increase the intra-ventricular pressure until it equals the intra-arterial; just as soon as the pressure is greater than the intra-arterial, the flow of blood begins.

This looks plausible, and coincides with this theory, but is not true; yet we will not stop here to disprove it. Let us now consider what takes place during the second tenth.

We have for consideration an intra-arterial column of blood under a certain degree of pressure, separated by the semilunar valvular wall from the intra-ventricular mass of blood which possesses a rapidly increasing degree of pressure, as the ventricle has already been $\frac{1}{10}$ of a second in systole, and is about to begin its second $\frac{1}{10}$ stage. On the ventricular side of the valve, the point offering least resistance to the mass of blood in the ventricle, is the center of the valve, containing the nodules of Arantius. This corresponds to the axis of the current on the aortic side of the valve, and is the point of least pressure within this column of blood. The blood must, therefore, enter the aorta through the valvular opening, wedge-shaped. It pries open the valve, presses the segments to the sides of the aorta, and finally passes entirely within the borders of this artery.

This mass, or column of blood, could not do this unless it were under greater pressure than the blood on the aortic side of the valve; therefore, it must push the segments before it and towards the walls of the artery. These segments must first assume a parallel position to the course of the blood, or blood-current; and second, they must be forced *towards* the walls of the artery. In other words, the blood must pass along the entire surface of the segments to enter the aorta.

Let us now consider whether the segments are forced to come in apposition to the wall of the aorta, or only so far that there remains more or less space between valve and wall, thereby allowing a certain quantity of blood to remain.

To repeat: With a straight-moving current the segments are parallel to the current, are forced towards the wall of the artery, either entirely to the wall and in apposition with it, or at some distance from it. Let us see now how many absolute impossibilities we shall be compelled to admit possible; how constantly and how often our mathematical axioms will be sinned against during this short $\frac{5}{6}$ of a second.

The blood on the aortic side of the valve is constantly flowing forward, therefore the least resistance is forward, and in the longitudinal axis of the artery. The blood from the ventricle should, therefore, flow forward instead of pressing the segments to the wall. If the blood flow forward so fast there would be a vacuum at the root of the artery before the next systole of the ventricle. In fact, we would need no valve whatsoever. To prove the error of this deduction, we have the fact that the aorta bulges out, gets wider as well as longer; therefore, the blood finds it easier to dilate the aorta, than to push forward the column of blood that is within the aorta at the moment of its ingress.

For this incoming mass of blood to dilate the aorta, it must first force the valve open; then, pushing against the wall of the valve, it exerts pressure against the blood on the arterial side of the valve. There being nothing to prevent, most of this blood is forced into the lumen of the artery. The valve would be pressed against the wall of the artery if the valve itself offer no resistance. As the valve can be stretched to form a floor to the aortic column of blood, it most certainly can offer no perceptible resistance.

We are, therefore, compelled to conclude that a straight current column of blood, which enters the aorta with such force as to cause the aorta, including the sinuses of Valsalva, to bulge out still more, must force out the blood situated between the valve and the wall, and the valve must lie in apposition to these walls at the moment of cessation of the flow of blood from the ventricle into the aorta. But the valve is closed at this moment; accordingly, the valve is open, pressed against the outstretched wall and closed, forming a floor to the column of blood in the aorta, at the same time—an impossibility—therefore the theory is wrong.

For the sake of argument, we shall admit that in some way or other, beyond our understanding and knowledge, the blood does remain between the valve and the aortic wall. We are then in a worse predicament, for if the segments are standing parallel with the wall, they most certainly are not closed simultaneously with the cessation of this current, but after it, and accordingly are open and closed at the same time. Secondly, we have admitted as existing that which we know must not exist, as being present at every systole of the heart that which we know is not only deleterious to health, but fatal to life. We have admitted that that portion of the blood situated between the valve and the wall of the aorta remains motionless during the passage of the blood into the aorta. Again, we are compelled to draw on our imagination and faith for a power to close the valve. We must bring in, so to speak, a possible fourth or fifth dimension, for we neither know of nor see any characteristic of the valve, nor mechanism at, nor near by, to close the valve without the loss of so much time that the ventricle would not only be in diastole, but almost in systole again.

Physiologists teach that the intra-arterial pressure closes the valve. They teach and we know that the blood flows in the direction of least resistance from greater pressure towards less; that the blood that is entering the aorta is under greater pressure than that which is in the artery. The greater pressure, therefore, lies between the segments and prevents the valve from closing unless it possess a mechanism of its own, such as a steel spring; that is to say, our present teaching is that the intra-arterial pressure closes the semilunar valve, and also that that portion of the blood between the segments is under greater intra-arterial pressure than the portion between the segment and the artery; therefore, the segments work in the direction of greatest resistance—the impossible possible.

Again, if the intra-arterial pressure from the arterial walls close the valve, the valve must close after the walls begin to contract; but it must close before the systole of the aorta, otherwise blood would regurgitate, the valve would not be perfect; so that the intra-arterial pressure from the walls of the aorta does not close the valve. There must, however, be some power that closes the valve before the walls can possibly exert any action to close it.

If the intra-arterial pressure closes the valve without regurgitation of the blood, it can only do it by forcing the blood which lies between the segments up in the direction of the aortic current—that is, away from the ventricle. This would lessen the diameter or calibre of the artery at this point by nearly one-half, before the ventricle dared to cease its systole; this is not a fact. It is an ascertained fact that the valve is closed before the calibre of the artery is lessened, and that coincidently with the lessening of the diameter of the artery the ventricle dilates. Imagine the wear and tear on the valve, this delicate duplicature of the serous membrane of the arteries, if used to force this mass of blood forward! And how disadvantageously the segments are placed and arranged for such work! At the closure of the valve we hear a musical sound. No musical sound could be produced by the valve if the cause were a column of blood pressing the valve against the contracted ventricle. Sound presupposes wave motion, which presupposes a certain amount of freedom or alternating pressure, anything except a great intra-arterial pressure on one side and a ventricle in systole on the other side of the valve. In short, the theory is wrong, for the sound-producing valve must have equal, or nearly equal, pressure on both sides of it.

Let us suppose physiologists correct, and the artery in diastole with the valve closed—that is to say, at the cessation of the current of blood from the ventricle into the aorta, the valve closes. If we accept the straight-current theory, we are again confronted with the improbable and the impossible. First, we are confronted with the statement that the intra-arterial pressure is exerted on the contents of the aorta, and the blood finding its progress prevented in all directions save one, flows in that direction—forwards. During this time ($\frac{22}{30}$ of a second), the valve is closed and forms a floor for the column of blood in the artery; therefore, during all this time its central portion must remain quiet, for, if it recede from the valve, it will either open it or a vacuum will take place, and if it remain quiet for $\frac{44}{60}$ of a second out of every $\frac{50}{60}$, precipitation at least, if not coagulation, would or is liable to take place.

With the straight-current theory, the greatest intra-arterial pressure is on the same line or axis as that of the least pressure, and at the one time; the latter forward in the axis of the current, the former backward against the valve—an impossibility. This delicate valve, capable of so little resistance, is asked to stand equal pressure with the walls of the aorta—another impossibility.

The lengthening of the aorta to receive the ventricular contents at systole of the ventricle, would tend to bring the aortic current to a standstill with a straight-current theory. And the coronary arteries would, instead of receiving blood at systole of the ventricle, return blood into the aortic cavity at systole of the heart, if the segments were not pressed against the wall.

We find that for almost the entire $\frac{25}{30}$ of a second, in which the revolution of the heart takes place, we have had to assume a continuous series of impossibilities that we know do not exist. We had to suppose the valve open, when we know it must have been closed; the segments pressed against the wall, when we know they must not be, in order to have them work. We are asked to believe that part of the current of blood, in the ascending portion of the arch of the aorta, stops flowing for $\frac{22}{30}$ out of every $\frac{25}{30}$ of a second. We reason the aorta is in systole, when we know it must be in diastole. In short, the straight-current theory and the statements made by physiologists to-day, as regards the action of the semilunar valve, are erroneous in every respect.

Let me, in conclusion, ask whether it is probable or possible, that a piece of mechanism so perfect, so ingeniously and intelligently contrived, to take advantage of every iota of time, space, and the laws of hydro-mechanics in its method of action, could err so palpably, and that its valves should be so arranged that the blood would of necessity have to flow along the entire length of the segments before passing them?

Why not have the segments lie at right angles to the current of blood, so that the blood would only have to pass over the edge to enter the lumen of the aorta; and have the segments of the valve open and close, like folding doors, to save time? The straight-current theory is wrong. With such a current, the semilunar valve would act as uselessly as a sieve. We must accept the semilunar valve, therefore we must drop the straight-current theory.

Taking up the rotary-moving current theory, I propose to prove that it is according to this theory that the blood flows through the semilunar valve, without regurgitating, and the valve works perfectly and automatically.

To recall the topographical anatomy of the left ventricle, aorta, and aortic semilunar valve: The cavity of the left ventricle and the cavity of the ascending portion of the arch of the aorta, can be described as two twisted cones placed with bases together, but separated by the twisted semilunar valve, which, when closed, acts as a diaphragm between the two cavities. The left ventricle, in its relation to the aorta, is a force-producing cone only. The aorta is simply a receptacle, so far as its relation to the force-producing cone is concerned. The valve is for the purpose of preventing regurgitation. From the foregoing, it is self-evident that the blood flows from the ventricle, the force-producer, into the aorta, the receptacle, and in its course must pass beyond the semilunar valve, to go from one cavity into the other.

The first, or force-cone, is composed of muscular walls and sends the blood from apex towards base. The second, or receptive cone, is composed mainly of musculo-elastic walls, and receives blood at its base and forwards it toward its truncated apex. The diaphragm, or valve, separating the two cones, is a passive agent only, and is mainly composed of the serous lining, a continuation of the aortic

The aorta is constantly filled, even distended, with a moving column of blood, which is consequently under pressure. The force-cone, the ventricle, fills itself slowly and empties itself rapidly; its contents are, therefore, alternately under pressure, and without any or under negative pressure.

The semilunar valve is a passive agent, possessing no mechanism; and, therefore, must open and close through forces from without; in this case, being acted upon by the blood. The blood must open the valve and close it without ceasing in its onward course. No portion of the blood in the receptive cone must cease its progressive movement, even for an instant, whether the valve be in the act of opening or open, in the act of closing or closed. In other words, the blood must close the valve and at the same time keep moving away from the valve. Nor must the column of blood entering the aorta from the ventricle leave any space unfilled between it and the blood already in the aorta, nor cease moving forward.

Immediately after the semilunar valve has closed, we find the ventricle still in systole and about to begin its diastole. The aorta is in extreme diastole, having just received the blood, and, therefore, at this moment containing the maximum amount. This column of blood is revolving on its axis. That portion contained in the ascending portion of the arch of the aorta, possesses the form of a spirally twisted truncated cone, the broad base formed by the semilunar valve, the apex in juxtaposition to and forming the beginning of the transverse portion of the arch of the aorta, and the sides of the cone bounded by the aortic walls.

This cone of blood is moving forward as a whole, because the aorta is shortening. It is getting thinner, that is, the blood is moving forward along the lumen of the artery. This is caused by the resistance of the contracting aortic wall. In other words, the resultant of the rotary motion and resistance of the walls of this cone is necessarily a forward motion. The rotary motion and centrifugal force are transformed into propulsive force and heat. The rotation is transformed into a spiral, and the spiral, with a short distance between the curves, to one with a long distance. The very act of lengthening and widening of the aorta, tends to propel the blood forward.

The segments of the semilunar valve do not present a flat surface to the ventricular opening, each segment bulges out like an overdistended pocket, until all touch one another with their facing surfaces, the surfaces that are directed toward the axis of the artery and not their edges or borders. These edges, or free borders, lie in the direction of the current of the blood. The nodules of Arantius project into the lumen of the artery and are lightly locked together.

The weight and intra-arterial pressure of the blood force the segments down and bulge them out, while the rotary current tends to cause a vacuum in the center and to draw the nodules in the direction of the current. Therefore, there is no pressure on the nodules and center of the valve from the direction of the axis of the current. The nodules curve upward with the current, and are pressed and twisted or locked together, not locked to prevent regurgitation, but to present a slight hindrance to the blood from the ventricle at its systole, the systole of the apex of the left ventricle.

I have now described the valve during the time it is closed. Meanwhile the ventricle has become filled with blood and is about to contract. Simultaneously with the beginning of systole, the semilunar valve opens, a small quantity of blood passes from the ventricle into the aorta, and the aortic and ventricular contents form one continuous column.

Let us consider the condition and governing circumstances of the ventricle as it is about to contract. We have before us a hollow muscular organ, whose shape and consistency it is needless to describe here. It is filled with nearly its own weight of blood and is ready to contract. It contracts from both base and apex toward a common center. Other things being equal, that portion which has the least resistance to overcome and the best purchase to contract on, will contract first. Therefore, if the same part has the best purchase and the least resistance, and contains by far the smallest volume, it will contract first.

The apex has perhaps forty times its own weight, the base of the heart and contents, as a purchase to contract on. It has, perhaps, $\frac{1}{40}$ the volume of the base. The apex, at its contraction, need move but the central portion of the mass of blood, and, therefore, has little or no friction to overcome. The apex, therefore, contracts before the base can possibly begin. The very action of the heart proves my deductions to be right. It would be absurd to think that the basal portion of the ventricle would hold on to the apex and pull the aorta down; or, that after the base has contracted and the ventricle is nearly empty, the apex would give a useless twirl. The apex contracts, the cavity of the ventricle is lessened. During this moment, the blood in the ventricle starts to flow in the direction of least resistance.

The mitral valve, floating on the top of this mass of blood gently closes, being nearer the apex than the semilunar valve. The tendinous chords straighten out. Then, the semilunar valve opens.

Here we see another purpose for the nodules of Arantius and the little lunated thin portion of the segments just back of the nodules. The nodules are locked, not to resist the intra-arterial pressure, but to offer a double assurance that the mitral valve shall close before the semilunar valve opens, even if the former is nearer the apex, the initial cause of the operating force.

Coincident with the opening of the semilunar valve, the intra-aortic pressure is transported into the mass of blood in the ventricle. The aortic and ventricular contents now form one harmonious whole or column of blood. The semilunar valve is also at rest, simultaneously with the establishment of the intra-arterial pressure within the ventricle. The segments cease to bulge; they flutter on different planes within this one column of blood. The nodules, acting as floats, seek the center of the column, but are kept apart by the current.

This column of blood extends from the bifurcation of the aorta to the contracted apex of the ventricle. It is incompressible and held in place by the walls of the distended elastic aorta, and, therefore, under great pressure. For all practical purposes, it may be considered as a solid column in constant motion, possessing the form of a shepherd's staff. That portion of the aorta attached to the vertebral column is capable of movement only in directions at a tangent to its longitudinal axis. The arch of the aorta and left ventricle portion of this column can be compared to the crook of the staff. It is comparatively free from the vertebral column, and is capable of movement in all directions. The imaginary axis of this column of blood, if projected, would reach to the capillaries at one end and terminate in the ventricle at the other

The left ventricle uses this revolving column of blood as a pillar, or fulcrum, over which it pulls the aorta. The apex portion of the ventricle forwards the blood. Immediately after the semilunar valve opens, the base of the ventricle contracts. It does not eject its contents; it pulls the aorta over this column of blood, at the same time causing the ventricular cavity to disappear. This disappearance is helped by the muscular columns rotating around one another. This column of blood contains the semilunar valve at rest, but lying transversely to the long axis of this column, and having equal pressure on both sides of it.

As the ventricle pulls the aorta over this column, the blood finds it easier to lengthen and widen the aorta than to force the aortic blood forward. As the rotating blood passes from a narrow into a wider conduit, its rotary motion is greatly increased, and, in consequence, its centrifugal power. This naturally increases the tendency to a vacuum in the center of the column. The nodules of Arantius are drawn toward this center in direct ratio as the aorta widens. In other words, with the increased centrifugal power at the mouth of the aorta, the segments of the semilunar valve are more powerfully drawn together, and the passage of the blood from the ventricle into the aorta prevents the valve from closing. As the volume of blood on the ventricular side decreases, the segments float nearer together, and as the last molecule of blood passes through, the valve closes. There is nothing to resist the centrifugal force which is constantly exerting itself to close the valve. The ventricle remains in systole $\frac{1}{10}$ of a second after the valve closes, to enable the valve to change and perfect its form.

We therefore see that the valve closes before the intra-arterial pressure can possibly come to bear. That is to say, the semilunar valve closes automatically and coincidently with the cessation of the passage of blood through the aortic opening, and before the intra-arterial pressure can possibly come into play. The semilunar valve is constantly trying to close, and necessarily does close, as soon as the current ceases. It closes with a musical sound, as it is surrounded by the revolving blood column. It is in the act of closing toward the end of systole of the ventricle, and while the aorta is still dilating. It is closed before the aorta can begin to contract. It is in the act of closing and closed at a time when, according to our present accepted straight-current theory, the segments of the valve would be furthest apart.

Hitherto, the heart has been credited with the wonderful and awe-inspiring feat of lifting millions of foot tons, but this is not so. The heart is not a coarse, powerful, common laborer. The ingenuity, wisdom, and intelligence displayed in the anatomy and physiology of the heart, exceeds that of the eye or ear, those wonderful organs so admired, because so well understood, just as far as the necessity of the heart exceeds that of the eye or ear.

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SCOLIOSIS; ITS ETIOLOGY AND TREATMENT.

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Read before the San Francisco County Medical Society.

Scoliosis, or rotary lateral curvature of the spine, is one of the most obstinate of pure deformities. It has been of interest to me partly because of this obstinacy, and I ask you to listen to a brief resume of its etiology, character, and treatment, the *technique* of which last I shall be able to show you.

All theories advanced to explain the causation, no matter to what tissue the first offense is charged, lead to the assumption of a lack of correlation between the parts supporting and the parts supported as the chief cause of the deformity. This is at once obvious in the consideration of the osseous theory, in which the vertebræ are supposed—from some cause not definitely agreed on, though all suspect rachitis—to become softened, resulting in structural weakness and inability to support the superincumbent weight. Sequent to this inability is a functional distortion of the spine, a position assumed instinctively by the subject, causing an oblique pressure on each of the displaced vertebræ, and the development, if the distortion persists long enough, of the typical deformity.

The same course of events follows if the muscles are originally at fault. The erect position of the spinal column being due to a balance of muscular force on all sides, it is plain that anything disturbing this balance, and so giving a preponderance of force on one side or the other, will also at once cause a functional distortion, and again place the vertebræ in such a position that the force of the superincumbent weight will fall on them obliquely. Of course cases of muscular paresis or paralysis due to spinal cord lesions are identical, mechanically, with cases of primary muscular weakness.

Reference here may be made to the experiments of Bradford and Lovett with the body of a child, to determine the effect on the spine of weight, or downward pressure. The legs and pelvis of the cadaver were firmly imbedded in plaster of paris, and on the head and shoulders was put a weight so great that the muscles and ligaments were unable to hold the spine erect in its central position, and it assumed that of a scoliotic spine, demonstrating that lateral curvature "results from downward pressure, but downward pressure applied obliquely upon some portion of the spinal column." This is, apparently, equally true if an absolutely too great weight is put on a normal spine—as on the spine of the cadaver in this experiment—or a relatively too great weight is put upon a spine weakened in itself or its supports, as in the cases that occur in life.

The special value of oblique force in the production of the deformity is easily understood if the position and relations of the vertebræ in the trunk be considered. The bodies of the vertebræ, placed in a vertical series, are attached to each other by longitudinal ligaments only, and receive no support on either side. As soon as a body is moved from its original central position in the trunk, the force of the superincumbent weight falls on it obliquely. This oblique force is resolved naturally into a vertical force, which alone supports the weight, and a lateral force which always thrusts the body further and further away from the mesial plane, tending constantly to increase of the deformity.

Now, with the increase of the deformity there is increase in the amount and in the mechanical advantage of the lateral force, and the bodies, considered alone, would be quite at its mercy, and would be surely driven further away from the center, and even entirely out of the vertebral series. The sole restraining force comes under these conditions from the interarticulation of the articular processes, the articulations with the ribs, and the imbedding of the transverse and spinous processes in the muscles which form, with the bones, the wall of the thorax. It is the isolation of one portion of the bone and the attachments of the other that prevent the whole bone being moved equally by the deforming force from its proper position, and the attachments, in their resistance, force it to make two separate rotary movements around two separate axes.

The chief and initial rotation is around an eccentric vertical axis, situated outside the trunk, posterior to the spinous processes, and is the result of the lateral thrust of the oblique force acting with less opposition on the isolated body than on the attached arch and processes. The secondary rotation is around an axis which passes through the tip of the spinous process and the middle of the front of the body, an axis that is antero-posterior to the bone. That this rotation is necessary is evident if we consider that the series of bones is unbroken, from the vertebra most displaced to the bones that are still normally located both above and below, for above, the under surface of each vertebræ has moved a little more than the upper surface, while below, the upper surface is the one that has moved most, and this inequality of motion rotates the entire bone around its antero-posterior axis, the direction of the rotation of the bones above being the reverse of that of the bones below.

It is the first of these two rotations that causes the distortion of the trunk and produces the appearance of lateral curvature; the second is incidental and non-deforming.

While the restraint of the attachments of the arch and processes is thus evident, it can never be sufficiently great to alone control the position of the body, for the deforming force, as applied to the body, has the long arm of the lever to operate on as compared with any force applied to or developed in the location of the articular and transverse processes, the latter being always in a position of mechanical disadvantage.

The theory of too great superincumbent weight on vertebræ of normal texture and resistance, and supported by normal muscles, has inherent difficulties. Cases are very far from rare of long-standing functional single curves due to inequality in the lengths of the legs, and in which the cause can be corrected by putting a sufficiently thick sole and heel under the shorter leg, or over-corrected by increasing the thickness of the sole and heel. These cases have no scoliosis in spite of the fact that the curvature of the spine, and the consequent oblique direction in which the force of the superincumbent weight has fallen on each vertebra, has continued for a long time, and even during the period of development. If the production of the deformity was simply a question of superincumbent weight and oblique pressure on the vertebræ, surely these cases would have scoliosis; as they have not, it seems necessary to assume some change, other than postural, in the muscular or osseous system, which disturbs the proper performance of function and leads finally to definite deformity.

Treatment has certain obvious limitations. So long as the condition is simply a functional distortion, and the patient can be put in the straight position, and can maintain that position easily, it should be possible to prevent the development of the definite deformity. If, however, the patient can be put in the straight position only with difficulty, and must make considerable effort to maintain it, it is quite certain that the case has passed the stage of functional distortion, and that changes in the shapes of the bones have occurred. In such a case it may be possible, but is not probable, that the deformity may be lessened and the improvement be maintained. Failing this, it is probable that increase of the deformity may be prevented, and the case be maintained in the condition in which it was first seen. At the same time, it must be acknowledged as possible, that, in spite of all efforts, the case may go on inevitably to a condition of extreme deformity.

The reasons for this are not far to seek. The curve, as we see it, is but the outward and minor manifestation of the inward and more serious displacement of the bodies of the vertebræ. Manifestly, these, jutting obliquely into the interior of the thorax, are quite inaccessible to any direct force, and their position can only be attacked indirectly through force applied to the ribs, bones of much less size and strength than the vertebræ, and neither situated in reference to, nor attached to, the latter, so as to act with mechanical advantage. It is because of this fact that it is always difficult, and almost always impossible, to lessen the amount of deformity which exists at the beginning of systematic treatment.

The methods employed in the treatment of this condition may, conveniently and chronologically, be divided into four classes:

1. Braces, corsets, and spinal supports.

2. Gymnastics.
3. The original Sayre method of auto-suspension.
4. The composite method.

The system of braces, corsets, or spinal supports, used alone, dates from the time of Ambroise Paré, or about 1650 A. D., and its obvious motive is the pressing of the deformed thorax back into the correct shape. The mechanical impossibility of finding on the trunk, leverage points for the development of sufficient force, has resulted in the method falling into practical disuse by all but very few orthopedists.

Treatment by gymnastics alone is based on the teachings of Ling, of Sweden, and has been elaborated and advocated by Mathias and Bernard Roth, of London, though the former gives the credit for its first methodical application to Professor Delpech, of Montpelier. The principal of the plan is, that in each case a position can be found which, for that case, puts the spine in its nearest approach to the normal. This is called the "keynote position" of that case, the usual scoliotic position being termed the "habitual position." The patient is then put through a series of selected exercises in the "keynote position," and there is finally developed a "best possible position," this representing the patient's nearest approach to the normal without the special aid of the "keynote."

The remainder consists in the gradual spinal development of the patient, so that the "best possible position" becomes the "habitual position." The method is especially applicable to the stage of functional distortion, and is not without value in mild cases of true scoliosis, where some bone deformity is present, but where the spine is still flexible and the thorax correspondingly plastic. While universally acknowledged to be of great value, gymnastics are not generally relied on, to the exclusion of other means, in the management of definite scoliosis, but they always occupy an important place in a broader scheme of treatment.

The original Sayre method consisted of auto-suspension to secure the utmost correction of the deformity, and then the application of a plaster of paris jacket. This was, in part, a combination of the two previous methods, and in many ways an improvement on them. The auto-suspension, however, proved, so far as the spine was concerned, a passive exercise, and the lack of active spinal exercise plus the support by the jacket, resulted in atrophy and weakness of the spinal muscles, so that no real therapeutic end was achieved.

The composite method combines the best elements of all the preceding plans, and adds the later development of forcible correction of the deformity by special apparatus, or scoliosis presses. Under this method, the patient is put through a selected series of exercises based on the plan of Roth; the scoliosis press is also used, and it is possible by it, with force from points outside the body, to so press on the protruding ribs of the convexity, as to correct, and even over correct, the deformity.

The chief point of attack is the contracted tissues, muscular and ligamentous, of the concavity, so that the rigidity of the curvature may be broken up and the muscles of the convexity, ostensibly the weaker group, may be given room and opportunity for development. Secondarily, the press is assumed to thrust the vertebral bodies, by force transmitted to them through the ribs and the transverse processes and arches, toward their normal place in the mesial plane. This latter effect is more apparent than real, it being much more likely that the ribs bend somewhat under the pressure, and that there is a lateral thrust of the whole vertebra toward the mesial plane of the body, with but little correction of the original rotations of the bone, in all but the mildest cases.

A retentive apparatus is added, and this may be a steel spinal support, but is more generally a jacket or corset, made of rigid material, such as plaster of paris bandage, leather, poroplastic felt, or celluloid, and moulded into shape over a corrected torso of the particular case. This apparatus is applied with full knowledge of its power to do harm by causing muscular weakness, but with gymnastic exercises faithfully carried out, that tendency is counteracted. The apparatus is thus made to serve a temporary, useful purpose, in holding a weak, or rigidly curved spine, as straight as it may be, while the muscular system may be developed to the point of maintaining that position without aid.

This combination of methods is not to be regarded as a "shotgun" prescription, but rather a selected combination, each method having its just value accredited it, and being used only in suitable cases. A "shotgun prescription" policy might even be forgiven in the light of the extreme obstinacy of this condition, and the horrible deformity it imposes on its victims.

The *technique* of the gymnastics is as follows: The patient must not wear tight clothing. The exercises are done in the "keynote position," and the patient is told to keep the fists tightly clenched. If this is done, it is accompanied by increased muscular tension all over the body, without further special effort. The "keynote" for the usual type of scoliosis, the right convex dorsal type, is with the left arm extended up from the shoulder, and with the right arm held out horizontally. During the exercises the patient must count aloud to prevent the holding of the breath and the consequent rigidity of the thorax.

1. *Keynote and counting aloud.* Bend the trunk forward and backward three times. Let the arms sink gradually to the sides where they hang with the hands palms forward. The patient holds the trunk unchanged from the keynote position, and stands as tall as possible. Then three deep breaths are taken.

2. *Keynote and counting aloud.* Bend the trunk sideways, to right and to left, three times. At first some assistance is needed to enable the patient to make a purely lateral motion, without any antero-posterior element. Then, arms by the sides, stand as tall as possible and take three deep breaths.

3. *Keynote and counting aloud.* Twisting to right and left three times. To accomplish this, the patient is told to look round, first in one direction and then in the other, as far as possible, the feet not being moved on the floor. In this way rotation to the limit is attained at every point where that motion is possible. Then, arms by the sides, stand as tall as possible, and take three deep breaths. Repeat all three times.

4. *Patient lies prone*, the feet are held down, and, arms in keynote position and counting aloud, the patient lifts the head and shoulders, and as much as possible of the trunk from the supporting surface; this lift is repeated three times. Then, three deep breaths, lying in the keynote position. Repeat three times.

5. *Patient lying prone*, keynote and counting aloud. First lifts the head and shoulders, as in the previous movement, then swings them laterally over towards the convexity of the curve as far as possible, comes back to the middle, and then lies down, repeating all three times. This motion is quite difficult, and is never given to a child until it has been prepared for it.

These are the chief specialized exercises. To them are added Indian club swinging, dumb bell choruses, chest weights, marching with sand bag on the head, and general physical culture. The result is almost invariably improvement in the general health; usually the curve is prevented from getting worse, while in some happy cases, it has been materially lessened.

Note.—These cases feel that they are straight when they are crooked, and the straight position feels to them a contorted one. In order to change this mental impression of position, I have put two mirrors in my gymnasium, and intend that new cases shall see their own deformities, and by visual sense be taught how to stand straightest. They shall also exercise before these mirrors to see how they look when they do badly, and how it feels to do it rightly.

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ITEMS IN AN EXPERIENCE OF OVER FORTY YEARS WITH CHLOROFORM AS AN ANESTHETIC.

By A. T. Hudson, M.D., Stockton, Cal.

Read before the California Northern District Medical Society.

It is nearly half a century since the application of ether in surgery was made by Dr. Morton, of Boston. In the fall of 1846 he came to the Albany Medical College to exhibit his discovery before the professors. He asked if any student had a tooth he wished extracted to step forward. Being one of the graduating class, I took the dental chair. He used a paper-cone inhaler, largely saturated with ether. It was applied firmly to the exclusion of air. It choked me very much, and the sense of strangulation was so great that the memory thereof has ever since restrained me in its use.

There are several serious objections to ether, which may be noted here:

1. Its distressing suffocation.
2. The longer time necessary to secure oblivion.
3. The less complete anesthesia.
4. Occasional total failure.
5. Frequent injury to the kidneys.
6. More persistent vomiting than when chloroform has been used.

The Surgeon-General's report of 1,210 cases of anesthesia during 1865, shows that the proportion of vomiting after chloroform was 14 per cent., and after ether 29 per cent. These statistics are based upon operative experience on soldiers whose normal resistive powers are greater than those of any other class of patients. In general practice, where half of our patients are women, the record against ether would be increased.

To these several negations there stands only one item in favor of ether—that is a smaller percentage of deaths, as reported by the German Surgical Congress and others. The correctness of these records may be altered much by later experience. It is well known that many patients die in hospitals within one to two days after etherization from vomiting and exhaustion. These cases are not reported as sequences of ether inhalation. If they were so recorded its favorable percentage would be far less. Chloroform is the most convenient and efficient anesthetic we possess. Why is it not the safest? Statistics do not give a sufficient answer.

Examining the 37 deaths under chloroform; from the surgical history of the war, we find that nearly all died before the operation began or was completed, and with a small quantity of the drug. Three of them were ascribed to heart disease; three to spasm; two to paralysis of the heart; two to overdose. Here are four specific reasons alleged for 11 cases of death, leaving 26 with no specific cause assigned.

One of these cases, No. 1,236, I saw and reported to the Surgeon-General's office. The soldier was private G. Budlinger, 76th Regiment, Ohio, 30 years old.

In the table of 1,210 cases of anesthesia taken from the above report, it is shown that the smallest quantity of chloroform given was ½ drachm. The largest quantity was 8 oz. Of ether the smallest quantity was 1 dr.; the greatest 21½ oz. No deaths are reported from these large amounts. The duration of anesthesia was 1 minute to 1 hour and 20 minutes under chloroform; and 1 minute to 3 hours and 55 minutes under ether. If the longer duration and the larger quantity of an anesthetic are not more fatal than a shorter time and a minimum quantity, to what shall we ascribe the most frequent cause of death under anesthetics?

The foregoing facts warrant the conclusion that two causes may be alleged as the chief factors in anesthetic mortality. One is maladministration, the other nervous dread or shock. For the latter danger, free stimulation with brandy to incipient intoxication, or a hypodermic of morphine, should precede the anesthetic, which should be given on an empty stomach. This, I believe, will fortify the system against anxiety and collapse.

Maladministration consist chiefly in faulty inhalers and overcrowding the anesthetic. In the army a folded napkin, or paper cone and sponge, were generally used. Dr. Sansome condemns the handkerchief method, because of the "utter ignorance we have of the strength of the vapor inhaled at a given time." A small Billroth inhaler, or a similar one like this, which can be charged with 15 to 20 drops at a time, is the best to control the anesthetic. It should be given slowly, so as to avoid all choking or suffocation. Then, if the chloroform is diluted with one-third of alcohol, I believe we have the best, the surest, and the safest anesthetic now at our command.

We often hear objections from patients, and sometimes from physicians, to the use of chloroform, in any form of heart disease. This danger is based more on fear than on fact, as statistics do not show it. The greatest mortality is among the healthy, the robust, and the middle aged. Dr. Sansome wrote, 30 years ago, that he had given chloroform in organic disease of the heart with safety. In this line my experience is limited, but I will note briefly two cases:

Mrs. W——, 45 years of age, desired to have a cancerous breast removed, and asked if I would give chloroform in valvular disease of the heart? I answered yes, and gave the drug successfully. A few months after, she desired to visit her daughter one morning, and on stepping to the street, she sank to the ground and expired.

The other case was a patient of Dr. F. R. Clark, of Stockton. He was a boy, about 15 years old, and suffering from organic disease of the heart. His abdomen was full, his breathing was labored, and he could not lie down. Tapping was the only thing to do, but he would not consent to that without chloroform. The anesthetic, preceded by brandy, was slowly given, in the half upright position. He went under it without a cough or nervous tremor, and a large amount of fluid was drawn off. This operation was repeated, at intervals, three times.

The fourth time it was required, his condition was most distressing. His abdomen and legs were distended, and the water filled the areolar tissue about his chest, arms, and throat. He could hardly articulate for want of breath. We saw that the end was near, and hesitated, saying to his mother that all had been done that we could do. She begged of us not to leave her poor boy, without giving him some relief—that they could not endure another such night. So we were induced to make an effort. Chloroform was given gently, and in a few minutes he began to sing. Then the inhaler was removed, and the trocar was inserted. Just as the fluid ceased to flow, the patient expired. Had death occurred at the first instead of the fourth operation, it would have been charged to chloroform.

One word upon stimulants: Most writers upon anesthesia speak indifferently, or not at all, in favor of stimulants preceding the drug, as a means of safety. I think they are of great value. I gave them in nearly all my operations in the army. Private Budlinger was one of the exceptions, which I have regretted ever since. Dr. Bedford Brown, of Alexandria, Va., gives a few clear pathological reasons for the use of stimulants with anesthesia. He had two patients, where the frontal bones were removed by compound fracture, which enabled him to see perfectly the changes going on under chloroform. "The invariable action of the anesthetic was to suppress hemorrhage, and positively to reduce the circulation in the brain and the arterial tension. When the patient was threatened with collapse from chloroform, stimulant injections promptly increased the circulation and arterial tension in the brain."

With these adjuncts, morphine and brandy, as heart and nerve sustainers, we may temper the lethal giant chloroform to the relief of all conditions of suffering humanity.

DEPARTMENTS.

OBSTETRICS, GYNECOLOGY AND PEDIATRICS.

By WALLACE A. BRIGGS, M.D., Sacramento, Cal., and
WM. FITCH CHENEY, B.A., M.D., Adjunct to the Chair of Obstetrics and Diseases of Women and Children, Cooper Medical College, San Francisco, Cal.

Sterility not Always the Woman's Fault.—VEDDER, of Christiana, Sweden, reports the results of examination of 310 married women, who had never been pregnant, though married at least one year. He was able to examine the husband also, in only 50 of these cases. As a result of his investigations, he draws the conclusion that in 70 per cent. of the cases, the husband is to blame for the sterility, either directly through azoöspermia or impotence, or indirectly through infecting his wife with gonorrhea. Only in 30 per cent. of the cases is the woman at fault, for the most part through neoplasms or atrophy of the uterus.—*Norsk Mag. for Lägevid*, 1895, No. 5.

Alumnol for Gonorrhea in Women.—CHOTZEN, of Breslau, recommends alumnol as a good astringent, that is also destructive to gonococci. In urethral and cervical gonorrhea of female patients, he employed injections of 2½ to 5 per cent. strength, and also suppositories of alumnol, 5 per cent. Fränkel, of Breslau, employs it in the form of 5 per cent. alumnol gauze for tamponade of the uterine cavity, and in 1 to 2 per cent. solution for vaginal douches.—*Centralbl. f. Gynäkol.*, October 26, 1895. [Alumnol is a white, readily soluble powder, considered almost a specific in gonorrheal endometritis. It is obtained by the action of sulphuric acid on naphthol-aluminium.—W. F. C.]

Try Pessaries First.—MENDES DE LEON, of Amsterdam, Holland, makes a plea for the more general use of pessaries for retroversion of the uterus, before operation is resorted to. He says: "Various operations, some of which may be qualified as ingenious and sometimes efficient, have so fascinated the profession as to cause the benefit which can be derived from mechanical treatment to be totally overlooked; and this is, in my opinion, a very grave mistake. In the treatment of displacemements, the pessary should always remain the prominent feature, and operative intervention be left for cases of obvious necessity." The writer then gives a review of the indications for pessaries, and discusses the best forms and the way in which they afford support.—*American Journal of Obstetrics*, February, 1896.

Difficult Labor after Ventro-fixation.—RICHARD C. NORRIS, in the course of a report of obstetric cases, at the Preston Retreat, mentions one, complicated by ventro-fixation of the uterus, performed two years prior to the delivery. The anterior uterine wall, throughout its entire extent, was apparently fixed to the abdominal wall. Almost the entire uterine sac was formed by the posterior uterine wall, which was dangerously thinned. The child's position was oblique, with the head lying under the spleen, the face anterior, and the feet resting behind the hypertrophied anterior uterine wall. Podalic version was not attempted, for fear of rupturing the uterus; but by external version the head was brought down and Tarnier's forceps applied. The cord prolapsed and could not be replaced. The infant was still-born.—*American Gynecological and Obstetrical Journal*, February, 1896.

Conservative Adnexa—Operations by the Vagina.—JACOBS, of Brussels, reports 17 cases where, on account of deep situation of the inflamed ovary or enlarged tube, or on account of adhesions, the vaginal route was chosen for operation. The cervix was seized and drawn forward, the vaginal wall opened transversely with the thermo-cautery, and the peritoneum with the finger. The opening with the Paquelin cautery has the advantage of avoiding hemorrhage, and also favors drainage by preventing too rapid healing of the wound. With the index finger, the organs in question were next freed from adhesions and drawn down, and of necessity were properly ligated and cut away. If their condition was such that they could be retained, they were pushed back high in the pelvic cavity, and retained there with a gauze tampon. Several times ignipuncture was employed for small cysts in the ovary, but the results were uncertain. The operations included 2 extra-uterine pregnancies, 7 cases of ovario-salpingitis, 3 of sclero-cystic ovaries, 1 hemato-salpynx, and 4 ovarian cysts, all without a death.—*Annal. de l'Institut Ste. Anne.*

SURGERY AND ANATOMY.

By T. W. Huntington, B.A., M.D., Surgeon Southern Pacific Company's Hospital, Sacramento, Cal., and

Geo. B. Somers, M.D., San Francisco, Cal.

The Imbrication Method of Operating on Inguinal Hernia.—Dr. Andrews, of Chicago, describes a method modified from Bassini's, as follows: The inguinal canal is opened up so as to expose the internal ring. The cord is lifted out of the canal and the inner opening closed by suturing the conjoined tendon and transversalis fascia to Poupart's ligament. The cord is then replaced, and the flap of the external aponeurosis, forming the inner pillar of the external ring, is placed behind the cord, thus supplementing the posterior wall of the canal. The lower flap of the aponeurosis of the external oblique forming the outer pillar of the ring is now placed over the cord and sutured, to form the anterior wall of the new inguinal canal.—*American Journal of Surgery and Gynecology,* December, 1895.

Radical Cure of Hernia by the Intra-Pelvic Method.—Reed gives the steps of this operation as follows: For inguinal hernia an incision is made through the skin and superficial fascia coincident with the axis of the tumor, from a point an inch below the external ring to a point two inches above the internal ring. The portion of the incision below the external ring is carried down to the hernial sac. The portion of the incision above the internal ring is carried through the abdominal wall, peritoneum and all. The sac, if adherent, is dissected loose, its contents reduced, and the sac itself invaginated upon the finger and returned into the pelvic cavity. It is next drawn out of the abdominal incision and bisected laterally and the flaps separated. The cord, now thoroughly exposed within the pelvis, is placed in the outer angle of the internal ring, the anterior and posterior pillars of which are stitched together by interrupted sutures of either tendon or chromacized catgut. The peritoneal flaps, formed by the bisection of the sac, are rolled together and are stitched by interrupted sutures into a protective pad, the natural position of which is directly over the internal ring. The cord is placed at the inner angle of the external ring, the anterior and posterior pillars of which are now stitched together by interrupted sutures of tendon or chromacized catgut. This done, the incision is to be closed by a laminated animal suture. This method of closing may be restricted to the peritoneum and to the fascia of the transversalis and oblique, but with these structures it is essential.—*Lancet Clinic.*

Case of Recovery After Trephining in Tuberculous Meningitis.—The following case is reported by Dr. James Kerr, of Bradford, England: The patient, aged 10 years, was a little light-haired boy, with neurotic and tuberculous family history. The illness began indefinitely, but developed into unmistakable tubercular meningitis. An operation was consented to. He was given chloroform and a trephine opening made one and a quarter inches above and the same distance behind the left meatus auditorius externus, removing a half-inch button. The dura mater bulged considerably; it was reflected, very little fluid escaping and the brain not pulsating. A fine canula was passed twice two and one-half inches in the direction of the ventricle; no fluid escaped, but the brain began to pulsate. Half a dozen silkworm gut strands were then with difficulty passed beneath the membranes toward the base; they seemed to go about one and three-quarter inches downward, the free end being brought out at the edge of the wound, which was then stitched, dusted with iodoform, and covered with dry sublimate wool. Sixty hours after the operation the dressing was changed, clear fluid draining off in small quantity. Dressing changed every third day. On the sixth day a convulsion occurred. On removing the dressing the wound and drainage-tube were found to be sealed with a hard scab; this was loosened and there was no further trouble. Drainage tube removed on seventeenth day. The patient is apparently well, but continues to have a temperature of from 99° to 100° F.—*Lancet*, October 28, 1895.

OPHTHALMOLOGY, OTOLOGY AND LARYNGOLOGY.

By Wm. Ellery Briggs, M.D., Sacramento, Cal.

Excision of the Ossicles and Membrane in Chronic Suppuration of the Middle Ear.—Dr. Richard Lake says that the occasion for removal of the diseased structures in chronic suppuration of the middle ear, are: (1) The cure of otherwise incurable suppuration. (2) Improvement of hearing power. (3) A minimizing of the risk of intra-cranial complications. (4) As a substitute for mastoid operation. Save in exceptional circumstances, no operation should be done unless the suppuration is over three months' standing, and has resisted antiseptic treatment, carefully carried out for that time, or unless this recurs frequently. To cure this disease, it is necessary, not only to remove polypi, granulations, and sequestra, but (and in this lies the key to the value of operative treatment) also to supply sufficient and free drainage. It is in cases in which the perforation is high up in the membrane, that the operation is most useful; for instance, in Shrapnell's membrane and the posterior superior quadrant. Antiseptic irrigation and good drainage cannot be secured with this condition. In case of the removal of the two larger ossicles when diseased, drainage is not always secured. But by removing the ossicles and the membrane, we have not always finished our work, as the attic and antral orifices are frequently obstructed by a mass of cicatricial bands or webs, with caseous pus and detritus ready to take active septic development. The operator should, therefore, remove with a curette, all the tissue from these regions, and combine this with efficient antiseptic irrigation, after using hydrogen peroxide. The author believes, if this line of treatment were thoroughly carried out, it would frequently obviate the necessity of a mastoid operation. This treatment more commonly improves the hearing than the long and often tedious treatment with antiseptics. To obtain the best results, both to hearing and recovery from the discharge, the whole of the membrane must be removed. Previous to operating, the canal should be well irrigated with a cool antiseptic solution.

The ear is then packed with cotton, soaked in 20 per cent. cocain solution, even if general anesthesia is used. The membrane is then completely divided at its attachment to the meatus. The tendon of the tensor tympani and the anterior ligament next require division. The membrane, with the malleus, is dragged down and out. Hemorrhage is checked by cocain and pressure. Extraction of the incus is troublesome, but can usually be accomplished by a crochet-shaped curette, which should also be used to clear out the attic. Great care must be taken not to injure the corda, which lies $\frac{1}{32}$ of an inch beneath the membrane. The treatment may not succeed in checking the discharge, in which case the antrum should be opened. The author advises the operation when careful treatment for six weeks to two months has failed to stop the discharge, or when the bone conduction is good and the impairment to hearing is very great after the cure of the discharge, under the following conditions: (1) Perforation in membrane Shrapnelli, with definite evidence of caries and deafness. (2) Perforation over foramen ovale, *i. e.*, in posterior superior quadrant. (3) Large perforation in the inferior half of the membrane, or including the whole membrane. (4) Perforation of the membrane Shrapnelli, with destruction of the anterior attic wall and disease of the ossicles. (5) When there is evidence of caries of the inner tympanic wall, as evidenced by lateral vertigo, etc., on syringing; dizziness alone being usually the effect of pressure on the stapes.—*Medical Press and Circular*, February 26, 1896.

DERMATOLOGY, SYPHILIS AND VENEREAL DISEASES.

The Bacillus of Chancroid.—COLUMBINI has been working on this subject, and publishes his results in a pamphlet. He finds that the bacillus of Ducrey and the strepto-bacillus of Unna are one and the same organism, characterized by being found in chains, by staining chiefly at the ends and not in the center, by being decolorized by Gram's or Kühne's method, by the difficulty of obtaining a pure culture, since a suitable nutritive medium could not be found, and by the rounded ends of the individual bacilli. The best staining agent was methylene blue. Inoculation into animals was uniformly negative. The bacillus is rarely found in bubonic pus.—*British Medical Journal*, March 7, 1896.

The Treatment of Soft Chancre.—The following treatment is recommended by *La Tribune Médicale:* Every morning the ulceration should be touched with a tampon of cotton wet in this solution: Menthol, gr. j; carbolic acid, gr. v; alcohol, ℥iss. Aristol is next dusted on, and a piece of absorbent cotton applied, thus completing the dressing. The patient is directed to wash the sore several times with carbolic-acid solution and to dust with aristol. All friction or causes of congestion are to be avoided. Phagedenic ulcer is touched with a solution made up of: Cocaine, gr. j; potassio-tartrate of iron, gr. xv; distilled water, ℥iss. It is next powdered with a mixture of 20 parts iodoform and 5 parts menthol; and potassio-tartrate of iron is given internally.—*Therapeutic Gazette*, March, 1896.

Syphilitic Testes in a New Born Infant.—At a recent meeting of the Pathological Society of London, MR. H. B. ROBINSON showed the testes of an infant, aged 4 months at death. The infant had well-marked congenital syphilis and had suffered from snuffles, with a rash on the nates and face. Before death, the child presented the typical "old man" appearance, and there were fissures at the angles of the mouth and sores on the face and buttocks. Both testes were of stony hardness and were enlarged and smooth. There was much thickening of the tunica albuginea and of the septa. Microscopically, there

was diffuse cellular infiltration, which, in places, had developed into firm fibrous tissue. Mr. Robinson showed, for Mr. Pitts, a second specimen from a child aged 5 years, in whom there was no definite history of congenital syphilis, but microscopic examination confirmed the syphilitic nature. Dr. Moore remarked on the very few illustrations of the internal morbid anatomy of congenital syphilis to be found in the London museums.—*Lancet*, March 7, 1896.

A Pernicious Disease of the Skin Not Previously Observed.—SPIEGLER reports (*Archiv für Dermatologie und Syphilis*,) a fatal case of skin-disease which was unlike any of the known forms. The patient was a well-nourished, powerfully built man, 38 years of age, who had never had any illness except an attack of syphilis eighteen years previously. Suddenly, without any demonstrable prodromes, he became ill with high fever and an erythematous eruption over the general surface. Upon this erythematous eruption, and in places upon the sound skin, serous and hemorrhagic bullæ appeared, the bottoms of which became necrotic. The process continued with high fever, and ended fatally at the end of three weeks. A bacteriological examination of the serous bullæ demonstrated the presence of an abundance of small rods, about one-third *u* long. Morphologically this bacterium could not be identified; and inoculations upon rabbits, guinea-pigs, and mice resulted negatively. Efforts to cultivate this bacterium from the blood were not successful.—*American Journal of Medical Science*, March, 1896.

MATERIA MEDICA AND THERAPEUTICS.

By WM. WATT KERR, M.A., M.B., C.M., Professor of Clinical Medicine, University of California, San Francisco.

The Digitalis Group and their Use in the Treatment of Diseases of the Heart.—The fundamental action of the members of this group is to increase the elasticity of muscular fiber so that it expands more slowly and contracts more perfectly. The effect of this upon a hollow muscle like the heart is that it dilates more slowly and empties itself more completely; residual accumulation is thus prevented and any dilating influences are counteracted. The heart is affected earlier and more powerfully than the other muscles, because the blood passes so frequently through it that it receives a larger dose of the drug in the same space of time. For a similar reason the muscular coat of the arterioles also gets a disproportionate share of the drug, and is early and strongly affected, though not nearly to the same extent as the heart, as the arterioles are only stimulated by the comparatively small quantity of blood passing to their own district. The more powerful contraction of the heart throws a larger wave of blood into the arteries, and as the effect of the increased elasticity of the muscular coat of the arterioles is to keep their caliber contracted, the blood escapes more slowly into the veins, it accumulates within the arteries and the blood pressure is raised. The effect of this is that every muscle in the body, and amongst them the myocardium, is flushed with blood at an increased pressure, and their nutrition and energy are improved and augmented by a more perfect metabolism. The action just described is that of digitalis itself; it seems probable that, besides the general action on the muscles, each of the members of this group has a common action on the heart, coupled with specific differences of its own. We know that digitalis acts powerfully upon the heart and blood vessels, while strophanthus acts powerfully upon the heart, and but slightly upon the muscular coats of the blood vessels. From the absence of any marked constriction of the arterioles, any rise in the blood pressure following the use of strophanthus must be due to ventricular action only; it is not, therefore, persistent, and can only feebly affect the muscular metabolism. Strophanthus is the only member of this group which has any pretensions to rival digitalis, yet it is so much more uncertain in its action, and so devoid of tonic properties, that we are not tempted to displace in its favor our own indigenous drug. For more than one hundred years digitalis has been known as a sovereign remedy for dropsy, but it is only within the last 20 years that its true properties have begun to be recognized. And even yet the profession is hampered in its use by the continued prevalence of the idea that digitalis is a most powerful but dangerous sedative to the heart—an idea that dates from those not-so-far-back days when digitalis was supposed to possess a number of distinct actions, all more or less antagonistic to each other. Now we know that digitalis is a tonic to the muscular fiber, and that it acts chiefly on the heart and on the muscular coat of the arteriole. From this action flow the benefits we obtain from the drug, while any unpleasantness that may follow its employment is due to the effects of an overdose, and for this the prescriber is to blame and not the drug. Digitalis in every form is absorbed with difficulty and excreted slowly; hence, if the dose is repeated at too short intervals the drug accumulates within the system, and ere long symptoms of poisoning appear. This faculty of accumulation, digitalis shares with many other drugs. Unfortunately, for its own reputation, its sphere of usefulness lies among a class of cases in which sudden death is an uncommon occurrence, and for many a day any sudden death of a patient taking digitalis was always ascribed to the cumulative action of the drug quite irrespective of the dose administered. As may be readily supposed, saturation is more easily brought about in small and anemic individuals than in those who are more bulky and plethoric. A tonic dose fitted to improve the nutrition and increase the energy of a feeble heart must always be a moderate one, and only repeated after the lapse of an interval sufficient to permit the preceding dose to be wholly excreted. In this way we secure that, at the end of the interval, there is a slight increase of the cardiac tone and energy, no digitalis remaining within the system. A usual and efficient dose to obtain this end is 1 gr. of powdered leaves, or an equivalent of any of the other preparations of the drug, every 12 hours. Such a dose may be continued safely for so long as may be required without risk of saturation. The feeble heart slowly and steadily improves, an imperceptible impulse becomes perceptible, and a feeble impulse a strong one, the patient meanwhile slowly acquiring an increasing sense of well-being and capacity for exertion. Such a tonic dose may be safely continued for years with nothing but increasing benefit to the patient. When we wish to contract young hearts which have dilated, or to excite diuresis, we must employ larger doses than such as are merely tonic, and as these larger doses have a cumulative effect the patient taking them requires a little more attention. The young dilated hearts we wish to contract are either such as follow various febrile affections occurring in early youth, or the flabby dilated hearts of those who are chlorotic or anemic. Such heart affections are often called functional, and the sufferers are supposed to grow out of them. But it is distressing to find, a few years later, that a want of timely attention has left this so-called functional affection as an obstacle to entering a profession or a hindrance to making provision for a family. When the heart is feeble and dilated it beats more quickly and less forcibly than in health, the ventricles empty themselves less completely, there is residual accumulation within their cavities, the arteries get depleted and the veins over filled. The result of this venous stasis is a soakage of all the tissues, and by persistence of all the conditions this ultimately collects as dropsy in the more dependent parts of the body and within its cavities. This dropsical accumulation is chiefly of consequence as an indication of heart failure, but in the tissue interspaces, and especially in the extremities, it constitutes an important peripheral obstruction to the circulation, increasing the work of an already failing heart. In such cases our forefathers employed digitalis empirically as a diuretic, sometimes giving as much as 10 to 20 grs. in powder or infusion every hour or two hours till nausea occurred. This sickness was accompanied by great diminution or suppression of urine, followed in two or three days by copious diuresis. Nowadays we know that digitalis has little, if any, action upon the kidneys, and that it produces diuresis by improving the condition of the heart and circulation. As the arteries fill and the veins empty the effusion is absorbed, the blood gets more watery, and this excess of water is excreted by the kidneys. A simple tonic dose acts as a diuretic by maintaining the integrity of the circulation; but when the heart is dilated, as well as feeble, and the accumulation of fluid is considerable, we require to act a little more energetically. A moderate dose, however, is quite sufficient; 3 gr. of digitalis, either in powder or infusion, every 8 hours, will be found a very efficient diuretic. Only exceptionally will a larger dose be required, and we always remember that the larger the dose, and the shorter the interval between the doses, the more watchful we must be for the symptoms of saturation. These symptoms may be looked for as soon as 30 to 40 grs. of the drug have been ingested; they are: diminution of a primary diuresis, slowing of the pulse, nausea, more rarely diarrhea. The diminution of diuresis may extend almost to suppression for two or three days after the drug has been discontinued; then diuresis recurs and may continue till all the fluid has been drained off, or it may be kept up by a moderate dose at regular intervals of 8 to 12 hours. These cumulative doses, by gradually improving the tone and elasticity of the myocardium, not only enable it to resist dilating influences, but in favorable circumstances are able to restore a dilated heart to its normal dimensions. Digitalis may be beneficially employed in all cases of heart failure whatever may be the cause or the nature of the concomitant lesion. In cases of valvular lesion, apparently fully compensated, digitalis may not be imperatively called for, but as in all such cases heart failure is steadily if slowly progressive, no harm can ever result from giving tonic doses of the remedy sufficient to maintain the normal elasticity of the myocardium. Some object to this, especially in certain forms of valvular lesion, from an idea that "the heart may be thus unduly stimulated, and the left ventricle hypertrophied beyond the needs of the circulation," so that it gets prematurely beyond the feeding power of its arteries, and the final and irremediable breakdown of compensation thus inaugurated and hastened by the very means employed to avert it. We have many drugs that cause atrophy; I know of none that cause hypertrophy. Hypertrophy of a muscular organ is the result of a call for increased exertion, and arises in no other way. The heart has a large reserve of energy, and even when called on for increased exertion does not at once hypertrophy; it is only when dilatation has occurred that the needful compensating hypertrophy follows. And so throughout the future progress of the affection the dilatation is ever in advance, the hypertrophy lags somewhat behind. Digitalis increases the elasticity and improves the nutrition of the myocardium, and enables it to withstand the dilating influences to which it is subjected. Far from unduly stimulating the heart and forcing upon it a needless hypertrophy, the tonic action of digitalis checks the progress of dilatation, retarding instead of precipitating hypertrophy. Corrigan long ago objected to the use of digitalis in aortic regurgitation, because he looked upon the drug as a pure sedative, and regarded the prolongation of the diastole as a source of increased danger to the heart. But the increased elasticity and energy that the myocardium acquires from digitalis is not only sufficient to enable it to resist the dilating influences to which it is exposed during the prolonged diastole, but is even adequate to produce a certain amount of contraction of the ventricular cavity, as shown by the recession of the apex upwards and to the right. Digitalis has no effect on the ingravescent systole of a moribund heart, but after the excessive dropsy has been diminished by purgation the drug often acts well, the rest of the fluid is absorbed, and the heart improved. In dilatation of the right ventricle secondary to mitral stenosis, or pulmonary emphysema, we cannot expect much from digitalis, as the obstruction to the circulation is incurable. Where the left ventricle is dilated from loss of arterial elasticity, digitalis acts badly unless combined with some vascular stimulant. Digitalis does positive harm in fatty heart. The best preparations of digitalis may be arranged in the following order: (1) The powdered leaf, not more than one year old. (2) The infusion. (3) The tincture. In conclusion, digitalis will confer more benefit on cases of cardiac disease than any other drug, not because it is a diuretic, sedative, or a cardiac stimulant, but because it maintains and improves its metabolism; for failure of metabolism is, under all circumstances, the great source of danger to that important organ.—GEORGE W. BALFOUR, M.D., in *British Medical Journal*, December 14, 1895.

MEDICINE AND PATHOLOGY.

By ALBERT ABRAMS, M.D., Professor of Pathology, Cooper Medical College, San Francisco.

Etiology of Mediastinal Tumors.—DR. FRANZ KARNIK presents the following contribution to this subject: The tumors localized in the mediastinum, owing to their rarity, seldom come under clinical observation. Of the pathological benign formations, the hypertrophic thymus develops *de norma* up to the second year of life, atrophying after the fifth year, and rapidly disappearing during the period of puberty, when it is replaced by fatty tissue; and for the diagnosis of a hyperplastic thymus growth, the microscopical disclosure of Hassal's corpuscles is a reliable guide. One must, further, enumerate among benign mediastinal tumors, lipomas, fibromas, dermoid cysts, teratomas, and hyperplastic lymph-glands—but these may here, as well as in other places, develop malignancy, and of this class is the case described by Virchow, of a large teratoma obtained from the cadaver of a man twenty-two years of age, in which were present carcinomatous and sarcomatous elements, besides a large number of embryonal striated muscle-fibres. Malignant mediastinal tumors in individuals of earlier age are usually sacromata, more rarely carcinomata. Mediastinal tumors, according to Feldmann, occur in men three times as frequently as in women, and are rarely encountered in either at an advanced age. Eichhorst found among thirty-five, Riegel among forty-one, and Thomayer among forty-six cases, but one case of more than sixty years. Riegel, moreover, asserts these tumors occur chiefly between the ages of twenty and thirty, which is corroborated by Thomayer; of the patients of the latter, eighteen were between twenty and thirty years of age, eight between thirty and forty, and five between fifty and sixty. All agree that the anterior mediastinum is, as a rule, the seat of malignant tumor, though Gluzihki and Impaccianti and others describe some few exceptions. Great difficulties are associated with the diagnosis of mediastinal tumors, for they may be simulated by neoplasms of the lung or pleura on the one hand, while on the other, mediastinal tumors do sometimes invade these organs; a malignant pulmonary tumor can only be accurately diagnosed when the microscope shows its elements in the sputum. Again, the diagnosis of a sarcomatous or carcinomatous pleuritis is rendered possible by establishing the presence of tumor-particles in the aspirated mass. It may happen casually, as reported by Unverricht, that the diagnosis is assured by the proliferation of the tumor from the point of puncture after puncture of the thorax; and it is by no means unusual for the neoplasm to proliferate and penetrate the thoracic wall so it can be felt under the skin. Almost insurmountable diagnostic difficulties become heaped one upon another when a comprehensive pleuritic exudate disguises the physical signs of the tumor. Clinically, a tumor is to be classified as mediastinal only when it appears as such anatomically. Returning now to the diagnosis: In interpreting a case, we examine one organ after the other, and, when the whole tableau is before us, compare it with familiar pathological conditions, and our conclusion is influenced by the histories of previous cases. The patient was 22 years old, suffered from dyspnea, palpitation of the heart, and later on from dysphagia and aphonia. The upper part of the sternum arched forward in a very striking manner, and the superficial veins of the upper right breast were not only greatly enlarged but very prominent. Cyanosis and edema of the right face and right hand also developed. Prior to death a stenotic bronchial murmur became audible at the level of the third intercostal space. At the autopsy, it became evident these symptoms arose from the development of an enormous mediastinal sarcoma, chiefly on the right side, with compression of the trachea. The right innominate vein was entirely compressed by the neoplasm, the right lung largely so. Litten succeeded in diagnosing mediastinal tumor during life in a man 22 years of age, in whom there developed, in addition to very distinct cyanosis, edema, dilatation of veins, etc.—ascribable to contraction of the upper vena cava—a hemorrhagic diathesis. At the point where puncture of the thorax was performed for the purpose of aspirating the pleuritic exudate, suggillations or ecchymoses developed; and the blood also assumed a leukemic character. At the autopsy, a large, small-celled sarcoma was found. All the cases cited were tumors of the anterior mediastinum. Hammer, however, publishes a case of tumor proceeding from the posterior mediastinum in a man 49 years old, supposed to be suffering from stenosis of the right bronchus. Autopsy revealed, at the level of the first dorsal vertebra, in the posterior mediastinum, a tumor the size of one's fist, proliferating forward and perforating the posterior tracheal wall at the level of the bifurcation; the left bronchus was compressed rather more than the right. The neoplasm was readily separated from the vertebra, and cross-section revealed a number of nodules as large as a hazelnut, which ultimately proved to be infiltrated lymphatic glands. Under the microscope, a large-celled alveolar sarcoma was revealed. With respect to treatment, permanent success can only be expected when the tumors are benign. Rochard describes one case of mediastinal fibroma which was permanently healed by operation. With malignant tumors, the practitioner must be content to treat symptoms. A diagnosis of mediastinal tumors is seldom definitely made,—it usually remains an open question,—owing to the complex and obscure symptoms. Often percussion will utterly fail to reveal the tumor, and there is every chance the malady will be mistaken for a form of pleuritis or pleuritic exudate. *Medical Age*, October 25, 1895.

MENTAL DISEASES AND MEDICAL JURISPRUDENCE.

By A. W. HOISHOLT, M.D., Assistant Physician, State Asylum for Insane,
Stockton. Cal.

Amentia.—DR. J. WIDEROE, of Christiana, has published two cases illustrating the complex of symptoms, which were first brought forward by the late Prof. Meynert, of Vienna, as constituting a psychosis, which he termed "amentia," and which is synonymous with Trille's "confusion," and with the delirium hallucinatorium of Mendel. Among the causes of the disease, all sorts of weakening influences have been mentioned; hereditary disposition may be present, but not necessarily so. It is usual for the prodromic state to be followed by a condition of confusion, incognizance of surroundings, incoherencies of speech, and muscular movements. The confusion is often an agitated one, and may alternate with stupor. Illusions of different kinds, but of a fleeting nature, may show themselves. Physically, symptoms of a general prostration are present. The average duration is 3 to 12 months, and about 50 to 60 per cent. recover. With regard to the differential diagnosis, some of the symptoms of amentia resemble the systematized illusions of acute paranoia, and the constant excitement or depression is to be differentiated from the exalted ideas and condition of depression met with in mania and melancholia.—*Norsk Mag. for Lägevid—Centralbl. f. innere Medicin.*

Contribution to the Doctrine of the Infectious Origin of Acute Delirium. DRS. BRANCHI and PICCINO have reported the results of their investigations of 8 cases of hallucinatory delirium (delirium acutum). In 7 cases recovery, or considerable improvement, took place; the eighth ended fatally. In the eighth case only did an examination of the blood reveal the presence of the bacilli, which the authors, in a previous article, have described as characteristic of the delirium bacillare (delirium grave). In all the other cases the blood contained micrococci arranged in chains, isolated, or in pairs. The usual aniline staining methods were employed. Among the microörganisms isolated from the blood, the authors noticed the streptococcus pyogeneus in 4 cases, and the staphylococcus aureus in 2. Drs. Branchi and Piccino summarize as follows with regard to the forms of mental disease, which clinically bear the aspect of delirium acutum, and have so far been described as such: Two subdivisions may be made of these forms—true delirium acutum, or delirium acutum verum sive bacillare, which is a very rare disease, and forms of sensory delirium and mania gravis, in which the bacteriological examination gives positive but indifferent findings. The delirium acutum bacillare differs from the other forms clinically by the greater intensity of the symptoms, by the adynamic stage which directly follows the stage of excitation, by the short course, and by exitus letalis, bacteriologically by the presence of a special bacillus in the blood and in the nerve-centers. Fever is not characteristic of this form, while the other forms of acute delirium are often febrile, the fever not being attributable to any other cause, as pneumonia, bronchitis, etc. Delirium acutum bacillare must be considered a grave infectious disease, in which the symptoms of excitement and of grave disturbances of consciousness are succeeded by symptoms of depression and a grave typhoid-like condition. In the other forms grave disturbances of nutrition are observed, accompanied by fever and other toxic effects on the functions of the central nervous system, the greatest disturbance of which consists in the hallucinatory delirium. The organic stupor must be considered as a secondary stage of the hallucinatory delirium, not only because exact observation always demonstrates the preexistence of a hallucinatory stage, but also by the identity of the bacteriological findings in the hallucinatory stage, and that of stupor.—*Annali di Neurologia.*—*Journal Nervous and Mental Disease.*

FORMULÆ.

By H. E. SANDERSON, M. D., Assistant Physician State Asylum for Insane, Stockton, Cal.

To Disguise the Taste of Codliver Oil:

Codliver oil	400 gm.
Freshly-roasted and ground coffee	20 gm.
Animal charcoal	10 gm.

Keep in a water-bath at 140° F. for 15 minutes; then in a stoppered flask for 2 or 3 days, shaking the mixture occasionally; after which, filter through paper. The oil will be limpid and light-colored, and will taste and smell strongly of coffee.—*Times and Register.*

For Tedious Labor from Rigid Os:

Tartar emetic, 10 gr. in ½ tumbler of water. Two teaspoonfuls every 10 or 15 minutes till emesis occurs. This produces free relaxation without diminution of expulsive efforts.—DR. DE MUNDÉ, in *Medical Record.*

Whooping Cough:

Bromoform	48 gtt.
Rectified spirits	4 drs.
Water (distilled)	1 oz.
Syrup of Tolu to make	3 oz.

S.—Teaspoonful in water every 4 hours. This must be kept in closed bottles, protected from the light.—*Polyclinic.*

Acute Coryza:

Chloral	gr. x.
Olei ricini	ʒiv.—M.

S.—Apply to mucous membrane, after cleansing it. This is said to arrest the secretion and calm the irritation, as well as the accompanying headache.—*Practitioner.*

Chilblains:

Resorcin.	
Ichthyol.	
Tannin	āā 30 gr.
Water	150 m.—M.

S.—Apply at night.—*Brooklyn Medical Journal.*

Insect Stings:

Aquæ ammonii	ʒi.
Collodion	m. xx.
Acid. salicylic	gr. ii.—M.

S.—Apply to each bite or sting.—*Popular Science News.*

Canker Spots:

Sps. camphoræ	ʒi.
Myrrh.	ʒii.
Glycerini	ʒv.—M.

S.—Mouth wash for canker.—*Practitioner.*

Chronic Eczema:

Liq. carbonis detergens	m.xx.
Hydrargyri ammoniati	gr.xx.
Unguenti zinci oxidi	℥ss.
Vaselini	℥ss.–M.

—*Practitioner.*

Gonorrheal Orchitis:

Guaiacol	℥iss.
Vaselin.	℥iv— M.

S.—Apply 3 or 4 times a day in the inguinal region, or on the internal surface of the thigh.—*Practitioner.*

Black Eye:

Tinct. or strong infusion of capsicum.
Mucilage of acacia.
Glycerine, of each a few drops.

S.—Apply with a small brush, making several coats.—*Medical Progress.*

Diarrhea in Infants:

Benzonaphthol.
Bismuth subnitrate.
Resorcin ....āā 1½ gr.—M

S.—Dose for a child 1 or 2 years old. To be taken every 2 hours until 6 have been taken.—Ewald.—*College and Clinical Record.*

For Chancroid:

Menthol................. 3 gr.
Carbolic acid.............15 gr.
Alcohol, 90 per cent..... 4 dr—M

S.—Apply on absorbent cotton every morning for 2 or 3 days. Keep it covered in the meantime with iodoform. —*La Tribune Médicale.*

PUBLIC HEALTH.

By W. R. Cluness, M.A., M.D., San Francisco, Cal.

Mortality for February, 1896.—The deaths registered in 49 town districts of the State, during the month of February, in a population of 722,470, correspond to an annual rate of 15.00 a thousand, the total mortality having been 910. 44 deaths were due to zymotic diseases, giving an annual rate of .68 a thousand. Of these, 9 were due to diphtheria, 3 to cholera infantum, 14 to typhoid fever, 3 to diarrhea and dysentery, 4 to cerebro-spinal fever, 1 to whooping cough, 5 to croup, 2 to intermittent and remittent fevers, 2 to scarlet fever, 1 to measles, and 0 to small-pox. 293 deaths resulted from diseases of the respiratory organs, giving an annual rate of 4.80 a thousand. Of these, 194 were due to consumption, 69 to pneumonia, 22 to bronchitis, and 8 to pulmonary congestion; the rate being, for consumption and pneumonia, 3.12 and 1.08, respectively. 87 deaths resulted from diseases of the heart, 5 from alcoholism, 3 from erysipelas, 37 from cancer, and 389 from causes not given. The average annual death rate from all causes occurring in the ten largest cities and towns in the State, and representing a population of 601,000, was 15.48. The highest rate for the month occurring in cities having a population of 10,000 or more inhabitants, was reported from San Francisco, the lowest from San Diego.

METEOROLOGY.

Summary for February—*Temperature.*—The normal temperature of the State for February is 48.8°; the average for February, 1896, was 52.0°, a departure from the normal of +3.2°. The highest monthly mean was 69.4°, at Ogilby; the lowest, 20°, at Bodie. The maximum temperature was 103° at Ogilby, on the 28th. The minimum, 12° below zero, at Bodie, on the 7th. The absolute range for the State was 115°. The greatest monthly range was 62°, at Salton, Chino and Tulare; the least, 15°, at Point Lobos.

Rainfall.—The normal precipitation for the State for February is 3.56 inches; the average for February, 1896, was 0.35 inches, a departure from the normal of −3.21 inches. The greatest monthly precipitation was 7.39 inches, at Crescent City; the least, nothing, at numerous places throughout the State.

Wind.—The prevailing direction of wind was westerly.

Unusual Features. — The unusual phenomena of the month were the extreme warmth and extreme dryness of the month: the former advanced at least two weeks earlier the blooming of certain fruit trees.—James A. Barwick, Director California Weather Service.

California—Tabular Statement for February, 1896.

STATIONS.	TEMPERATURE.						RAINFALL.		Mean Relative Humidity	WEATHER.			WIND.	FURNISHED BY—
	Mean	Highest	Lowest	Mean Highest	Mean Lowest	Mean Daily Range	No. of Days Rain Fell	Total Rainfall		No. of Clear Days	No. of Fair Days	No. of Cloudy Days	Prevailing Direction.	
Auburn ....Cal.	52.1	85	37	------	------	------	----	0.49	------	----	----	----	N.E.	Southern Pacific Company.
Berkeley......... "	50.3	70	39	61	46	15	3	0.36	77	25	1	3	N.	Obs. University of California.
Colfax............ "	49 2	75	32	------	------	------	----	0.93	------	----	----	----	N.E.	Southern Pacific Company.
Coronado........ "	61.0	79	43	70	52	18	2	0.02	58	20	4	5	N.E.	Hotel del Coronado.
Eureka........... "	48.4	69	34	56	41	15	8	4 53	83	9	17	3	N.W.	Weather Bureau Dept. Agr.
Fresno........... "	53.4	80	31	67	40	27	1	0.06	64	25	3	1	N.W.	" " "
Independence.. "	47.2	72	26	61	33	28	0	0.00	32	22	5	2	N.W.	" " "
Los Angeles.... "	59.6	88	36	74	46	28	1	T	56	21	5	3	N.E.	" " "
Monterey .... "	53.9	74	33	------	------	------	----	0.12	------	----	----	----	E.	Southern Pacific Company.
Napa............ "	54.7	77	34	69	41	28	3	0.25	74	20	6	3	N.	Station Napa Insane Asylum.
Oakland......... "	51.5	71	38	62	45	17	4	0.24	79	17	10	2	N.W.	Chabot Observatory.
Paso Robles.... "	48.0	72	26	63	34	29	1	0.02	------	25	3	1	N.	Agr. Experimental Station.
Red Bluff....... "	53.2	75	34	64	42	22	4	0.27	59	17	9	3	N.W.	Weather Bureau Dept. Agr.
Sacramento. ... "	54.0	72	38	63	45	18	2	0 09	69	16	12	1	N.W.	" " "
San Bernardino "	56 0	85	30	73	39	34	6	0.00	------	22	4	3	W.	A. K. Johnson, M. D
San Diego....... "	58.0	53	39	68	48	20	1	0.02	58	20	4	5	N.E.	Weather Bureau Dept. Agr.
San Francisco.. "	55.3	72	44	62	48	14	5	0.28	75	21	7	8	W.	" " "
Santa Barbara.. "	61.2	85	40	73	49	24	0	0.00	53	23	3	3	W.N.W.	Hugh D. Vail.
Santa Cruz...... "	55.0	89	33	62	39	33	1	0.25	------	27	1	1	W.	W. R. Springer.
Yuma...........A. T.	60.8	91	34	75	47	28	0	0.00	30	20	8	1	N.	Weather Bureau Dept. Agr.

Blank (----) indicates data missing.

Occidental Medical Times.

COMMUNICATIONS are invited from all parts of the world. When necessary to elucidate the text, illustrations will be furnished without cost to the author.

SACRAMENTO: APRIL, 1896.

THE STATE SOCIETY.

The twenty-sixth annual meeting of the State Society will be held in Odd Fellows' Hall, South Main street, between Second and Third streets, Los Angeles, April 21, 22 and 23. Any member who is in good standing in his local Society, and who has resided in the State for six months, is eligible for membership. Those who desire to join should at once forward their credentials, with the sum of $7, being $2 initiation fee and $5 annual dues, to the Chairman of the Board of Censors, DR. C. L. BARD, in care of the Secretary of the Society, DR. W. WATT KERR, 528 Sutter street, San Francisco, or to the Chairman of the Committee of Arrangements, DR. W. G. COCHRAN, Bradbury Block, Los Angeles.

There will be two sessions daily, a morning session, commencing at 9 A. M., and an afternoon session at 1:30 P. M. This plan has been adopted owing to the fact of the annual Fiesta or flower fete taking place in Los Angeles on the same days as the meeting of the Society. As all patriotic Los Angelaños should attend the festivities, and as many of the visitors certainly would, it has been deemed best to abandon the evening session, devoting that time to pleasure. As the Fiesta will make large demands on local hospitality, as well as providing a novel and prominent attraction, there will be only one medical entertainment. This will be held on Tuesday evening, April 21st, when a reception will be given by the Los Angeles County Medical Society and the Southern California District Medical Society at the new Turn Verein Hall, 323 South Main street, from 9 to 12 o'clock. Tickets will be furnished by the Entertainment Committee of the Los Angeles County Medical Society, of which DR. W. W. HITCHCOCK is Chairman. On Friday, April 24th, there will be an excursion to Echo Mountain over the Mount Lowe Railway, leaving Los Angeles, by Pasadena electric car, at 9 or 10 A. M. The fare for the round trip, including dinner at Echo Mountain House, will be $4.45.

The fact that there will be but two sessions renders it imperative for the efficient transaction of business that promptness in coming to order must prevail. A number of papers have been announced, and, on looking over the list, we feel certain that a large proportion of these will be presented. Their titles and the names of their authors indicate that they will be of interest, and that several of them will provoke discussion. For these reasons it will be absolutely impossible to transact the business of the Society if members are not in their places promptly at the appointed time. The afternoon of the second day has been devoted to the election of officers and such committee reports as usually do not occupy much time. At this session no voluntary papers have been placed on the programme. This provision has been made as the afternoon parade on that day will be the grand feature of the Fiesta.

We publish elsewhere the reduction in transportation rates, and it is probable that an extension of time of the usual "48 hours after adjournment" will be obtainable. Owing to the crowded condition of the city, it has been impossible to obtain reduced hotel rates. It is certain that Los Angeles will be filled to overflowing, and we can not too often impress upon intending visitors the necessity of securing accommodations in advance. Owing to the fact that but two applications for space from intending exhibitors were received, no provision has been made for the usual exhibition.

CALIFORNIA NORTHERN DISTRICT MEDICAL SOCIETY.

The semi-annual meeting of the California Northern District Medical Society was held at Red Bluff on Tuesday, March 10th. This is the first meeting that the Society has held at the northern end of its territory, the meeting at Stockton representing the southern extremity of the district. While the attendance was not large, the meeting was an eminently pleasant and successful one. For the District Society, Red Bluff possesses an historical interest. As Dr. Fife stated in his address, as Chairman of the Committee of Arrangements, many years ago a District Society, consisting of Colusa, Tehama, and Shasta counties was organized at Red Bluff, and, as he expressed it, he had hoped on this occasion to have brought all the surviving members of that Society into the new and larger organization. In this effort Dr. Fife was largely successful, and the Society certainly received an excellent welcome.

The hall in which the sessions were held was one of the most comfortable and certainly the best lighted of any that the Society has, so far, occupied. The papers were not numerous, but as each was discussed, and as the discussions were quite lengthy, there was ample material. At the close of the meeting a banquet was given at the Tremont Hotel by the local physicians, which was a very enjoyable affair. Red Bluff has an excellent hotel and an excellent hall, amply sufficient for a much larger gathering, and with a local profession earnestly imbued with the spirit of professional organization and hospitably inclined it will always be a desirable place for a meeting. The next, or annual meeting, will be held in Marysville, on October 10th.

It has no doubt engaged the attention of every member of the Society that the semi-annual or spring meetings are marked by a comparatively small attendance. This is undoubtedly due to the proximity of the State Society meeting. Many members who might wish to attend cannot be absent from their professional duties so shortly preceding the greater event. After much consideration it has seemed to us advisable to urge the consideration of a change in the time of holding our meetings so that the dates shall not conflict with other events which members of the District Society would be likely to attend. This could be accomplished by holding the semi-annual meeting in July or August, and the annual meeting in November or December. The subject should certainly be brought up for discussion at the next meeting. Under the by-laws it cannot be adopted unless submitted at a previous meeting. We believe, however, that under Article X of the by-laws the "previous meeting" mentioned may be a *special meeting*. If this were held at any time the amendment could then be discussed and adopted at the annual meeting at Marysville next October.

SAN JOAQUIN VALLEY MEDICAL SOCIETY.

In pursuance of a call for the purpose of organizing a District Medical Society in the San Joaquin Valley, a meeting was held at Fresno, on Monday, March 23, 1896. The call was issued by the following committee: W. N. SHERMAN, Merced; E. C. DUNN, Fresno; R. B. ROSSON, Tulare; J. A. MOORE, Hanford; A. J. PEDLAR, Fresno; C. E. BERNHARD, Visalia; T. E. TAGGART, Bakersfield; L. E. FELTON, Hanford. In accordance with this call a very successful and enthusiastic meeting was held, and the San Joaquin Valley Medical Society was duly organized, the following officers being elected for the ensuing year: President, L. E. FELTON, Hanford; First Vice-President, W. N. SHERMAN, Merced; Second Vice-President, T. E. TAGGART, Bakersfield; Third Vice-President, W. R. CHARLES, Lemoore; Secretary, C. A. ROGERS, Bakersfield; Assistant Secretary, C. E. DUNN, Fresno; Treasurer, W. F. MAUPIN, Fresno. Thirty-three physicians signed an agreement to become members of the Society, and to abide by its constitution and by-laws when they were adopted. The new Society has certainly started on its career most auspiciously. It occupies a large and growing section of California that has been rather neglected in the matter of medical organization. We hope the new Society will, in the near future, be the means of inviting the State Society to Fresno, an obligation which that flourishing city cannot much longer escape. In subsequent issues we shall publish a detailed report of the meeting, as well as the papers that were presented upon that occasion.

NOTES.

Dr. G. C. Simmons.

Dr. G. C. Simmons has resigned his position as collaborator in charge of the department of Dermatology, Syphilis and Venereal Diseases on the staff of this journal.

The New Medical Register.

The eighth edition of the Official Directory and Register of Physicians and Surgeons of the State of California has just been issued by the Board of Examiners. The plan of the work is identical with that of last year. The alphabetical arrangement of members in the county lists has been maintained, which was one of the most important changes made in the arrangement of the last Register. It is impossible to get out a work of this kind without errors. Some, as in the list of medical societies and boards of health, might, perhaps, be avoided, while others, particularly those relating to the location of licentiates, are unavoidable. The presswork of this edition is as good as that of recent years. The paper, however, is of poorer quality than is desirable in a volume of this character. We understand, however, that this was due to a misunderstanding which was not discovered until too late for correction.

La Fiesta de Los Angeles.

The Fiesta, which Los Angeles has now made a permanent annual celebration, will take place on April 22d to 25th. The programme, as announced, will be as follows: April 21. *Morning*—Preliminary ceremonies. *Afternoon*—Arrival of the heralds and advance guard of the Queen. Street procession. April 22. *Afternoon*—Street Parade in honor of the Queen. Floats indicative of progress and prosperity; Spanish riders, military, Chinese, Indians, etc. *Evening*—Grand concert at the Pavilion. April 23. *Afternoon*—Athletic performance at Athletic Park; Indian races and games; Tournament. *Evening*—Illuminated street pageant; Topic: "The Lands of the Sun." April 24. *Afternoon*—Children's entertainment; music and parade of children's floats; raising of flag on the great liberty pole. *Evening*—Bal masque at the Pavilion. April 25. *Afternoon*—The great flower parade of decorated equipages. *Evening*—Parade and carnival of the maskers; revelry of Comus. The celebrations of this character already held in Southern California have been very beautiful affairs, and it is to be presumed that this occasion will eclipse its predecessors.

Rates and Fares.

The following reduction in transportation rates is available for physicians and their families. The Southern Pacific and Santa Fé companies will give the usual one and one-third fare, round trip rate. Passengers must take a receipt from the agent, preserving same until countersigned by the Secretary at the meeting; and upon presentation to the ticket agent at Los Angeles a return ticket will be sold for one-third fare. Tickets can be purchased at any time before the meeting, but to secure the reduction the certificate must be presented not later than Monday evening, April 26th. It is, however, likely that this date may be further extended, but at present no positive announcement can be made. The return journey must be completed within two days from date of sale of ticket, and is good for a continuous trip only. Blank forms for railroad certificates will be issued to members. The Pacific Coast Steamship Company will give a one and one-half fare rate. This goes into effect April 17 and will be good until April 29. We have been informed that it has been impossible to obtain any reductions from the hotels in Los Angeles. This is due to the fact that their accommodations will be overtaxed by the crowds expected in that city during the Fiesta week, this event being coincident with the meeting of the State Society. In addition, there will be 500 hotel-men from different parts of the United States who will meet in convention in Los Angeles the same week.

The Reduction in the Fees of Medical Examiners for Life Insurance.

The work inaugurated by the Sacramento Society for Medical Improvement seems to be bearing abundant fruit. The original resolutions have now been adopted by a number of Societies on this Coast, and the fight has been taken up in Southern California by the *Southern California Practitioner*, and in the North, in Oregon, by the *Medical Sentinel*. Dr. Henry W. Coe, the editor of the latter journal, was instrumental, not long since, in organizing the Pacific Coast Association of Medical Examiners for Life Insurance, and this Association pledged its members to refuse acceptance of the proposed reduction in fees. Dr. Coe is, therefore, doubly committed to his position, and is making a good showing for the local profession. At the semi-annual meeting of the Northern District Medical Society the resolutions were unanimously adopted, the members present expressing themselves in very emphatic terms on the subject. Dr. J. S. Cameron alluded to the fact that he had recently been receiving inquiries from the New York Life regarding the status of members of the profession in Northern California. He believed that these inquiries related to physicians who were willing to do cut-rate work, and he had determined to inform the company that there was no physician in Northern California capable of making their examinations at the $3 rate. The resolutions have been adopted by the Los Angeles County Medical Society, and the new San Joaquin Valley Medical Society starts out well by adopting them at its first meeting. We publish elsewhere some communications on this subject, and in our next issue will note the action of the State Society on this very important question.

Voluntary Papers.

The following are the titles of voluntary papers, with the names of their authors, that have, so far, been received: "Public Management of Epidemic Diseases," A. W. Perry, San Francisco; "Some Examples of Drug and Food Adulteration," G. F. Hanson, San Francisco; "Erythema Caused by Mercurial Preparations," D. W. Montgomery, San Francisco; "Experimental Study of Reflex Emphysema and Hitherto Undescribed Affections," Albert Abrams, San Francisco; "A Consideration of Surgical Treatment of Uterine Displacements," H. Kreutzmann, San Francisco; "Pan Hysterectomy, with Cases," Wm. A. Edwards, San Diego; "Health of the School Girls," Charlotte B. Brown, San Francisco; "Rachitis in California," H. M. Sherman, San Francisco; "Foreign Bodies in the Alimentary Canal," Joseph Kurtz, Los Angeles; "Hydrocephalus," C. B. Nutting, Etna Mills; "Edson's Aseptolin," Washington Dodge, San Francisco; "Skin Diseases in Relation to Life Insurance Examiners," G. C. Simmons, Sacramento; "Injuries and Diseases of the Male Urethra," J. S. Sargent, Santa Rosa; "Epiphora," Rosamond L. Cox, San Francisco; "Ophthalmic Aphorisms," H. I. Jones, San

Francisco; "Trisometropia," W. F. Southard, San Francisco; "Cases of Urethral Structure with Unusual Complications," T. W. Huntington, Sacramento; "Work in Intestinal and Stomach Surgery," D. D. Crowley, Oakland; "Medical Education," J. R. Lainé, Sacramento; "Cerebral Localization in Relation to Surgical Operations," J. W. Robertson, Livermore; "Fleschzig Treatment of Epilepsy," H. E. Sanderson, Stockton; "The Care of Premature and Feeble Infants," Adelaide Brown, San Francisco; "A Case of Uterine Inversion," Walter Lindley, Los Angeles; "Purulent Catarrh of the Frontal Sinus, Its Surgical Treatment, with a Report of Three Cases," J. D. Arnold, San Francisco; "Utility of the Galvano-Cautery Loop in the Removal of Neoplasms from the Epiglottis and Adjacent Structures," M. C. O'Toole, San Francisco; "Treatment of Acute Inflammation of the Ethmoidal Cells and Frontal Sinuses," H. L. Wagner, San Francisco.

The Discussions.

The following are the standing committees with the names of the Chairmen and of the members who will open the discussions in the various sections. Wherever obtainable the title of the paper is also given: Clinical Medicine, W. Watt Kerr, San Francisco—"Tachycardia;" W. A. Edwards, San Diego. Surgery and Anatomy, C. L. Bard, Ventura—"The Climatic Advantages of Littoral Southern California;" ———. Obstetrics, C. A. von Hoffman, San Francisco—"Treatment of Puerperal Fever, the After Effects of Vagino-Fixation;" D. B. Van Slyck, Pasadena. Gynecology, Beverley McMonagle, San Francisco—"Ectopic Pregnancy, with Cases and Remarks;" W. W. Hitchcock, Los Angeles. Diseases of Children, F. W. D'Evelyn, San Francisco—"Sporadic Cretinism;" J. H. Utley, Los Angeles. Ophthalmology, Kaspar Pischl, San Francisco—"Syphilis of the Eye;" H. Bert Ellis, Los Angeles. Laryngology, Rhinology, and Otology, W. E. Hopkins, San Francisco—"The Value of the Operation for the Removal of One or More of the Bones of the Middle Ear, and a Report of Fifteen Cases;" W. R. Babcock, Los Angeles. Mental Diseases, Leo Newmark, San Francisco—"Certain Diseases of the Nervous System of Children Resulting from Difficult or Premature Birth;" H. G. Brainerd, Los Angeles. Dermatology and Genito-Urinary Diseases, George Chismore, San Francisco—"Some Points in the *Technique* of Litholapaxy;" Granville MacGowan, Los Angeles. State Medicine, H. S. Orme, Los Angeles; W. R. Cluness, San Francisco. Medical Topography, R. F. Rooney, Auburn—"The Climatology and Diseases of Placer County;" J. H. Davisson, Los Angeles. Botany, W. P. Gibbons, Alameda—"On the Origin and Distribution of Forest Trees of the Pacific Coast;" ———. Medical Jurisprudence, C. N. Ellinwood, San Francisco. Histology, J. H. Stallard, San Francisco. Pathology, Emmet Rixford, San Francisco—"Protozoic Infection, and a Further Study of Two Cases Reported in 1894;" George W. Lasher, Los Angeles. Medical Education and Legislation, R. H. Plummer, San Francisco—"A Revised Medical Bill;" J. H. Parkinson, Sacramento.

Dr. Hirschfelder's New Treatment for Consumption.

Some time since announcements were made in the San Francisco papers, notably in the *Examiner*, that Dr. J. O. Hirschfelder, of that city, had discovered and was then engaged in using a "new cure" for consumption. Dr. Hirschfelder promptly disclaimed any responsibility for this publication, asserting, as may readily be understood, that the method which he was trying was still under investigation, and that too short a time had elapsed to enable him to draw any conclusions from the results obtained. Newspaper notoriety of this character is always unpleasant for reputable members of the profession. On the other hand, Dr. Hirschfelder justly feels that the premature publication of any method which is as yet in the primitive or theoretical stage, cannot be regarded as advancing the cause of true science. In deference to the suggestions of his friends, however, Dr. Hirschfelder recently requested an investigation of the results which he had so far obtained. In accordance with this request, a committee, which will be regarded as thoroughly representative of the profession, examined some of Dr. Hirschfelder's cases, and signed the following statement:

> We, the undersigned committee, requested to examine the results obtained by Dr. Hirschfelder, in the treatment of tuberculosis by a method which he is now using, believe that the results are encouraging and warrant his further investigation; but suggest, that at present, it would be premature to bring the matter before the profession. This opinion is based upon the examination of nearly 20 cases treated by the method.
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> WM. WATT KERR, Prof. Clin. Med., University California.
> L. C. LANE, Prof. of Surgery, Cooper Medical College.
> C. N. ELLINWOOD, Prof. Clinical Surgery, Cooper Medical College.
> C. CUSHING, Prof. Gynecology, Cooper Medical College.
> HENRY GIBBONS, JR., Prof. Obstetrics, Cooper Medical College.
> San Francisco, March 15, 1896.

We expect that in due course the new method, with explicit details, will be presented to the profession, and meanwhile believe that Dr. Hirschfelder's idea in requesting an investigation at the hands of well known practitioners, will serve to strengthen his position. We may add, that full details of his treatment were embodied in a statement which was delivered to a third person some weeks ago, and which will be retained for verification in the future.

California Woman's Hospital.

The California Woman's Hospital, located at 3118 Sacramento street, San Francisco, which is devoted exclusively to the treatment of diseases peculiar to women, has, within the past year, at a cost of over $30,000, erected an annex to the main building. This contains several rooms of a superior character for patients desiring first-class accommodations, and a new operating room, which, with the necessary accessory rooms, has certainly been designed and built regardless of cost. This portion of the new building which will particularly interest our readers consists of an operating room proper, an etherizing room, a room in which dressings are kept in a sterilized condition, a room for microscopical work, and a dressing room for the operator and his assistants. All these apartments have been constructed upon the latest and most accepted ideas regarding antisepsis. The floor and walls of the operating room proper are of a peculiar dark green volcanic sandstone imported from Haddington Island, British Columbia. It takes a very smooth surface, and is said to be unaffected by the chemical agents used in antiseptic surgery. The basins and sinks in connection with the operating room are cut out of solid blocks of this material so as to reduce the number of seams and crevices to a minimum. The ceiling is of glass, and the doors are of iron, enamelled. The same materials are used in the adjunct rooms. In the basement is an engine operating two Sturdevant blowers, by means of which fresh air is forced into the operating rooms through screens containing sterilized cotton, and thence exhausted through an up-take connected with the smoke-stack of the furnace. Distilled water is furnished from a large tank above the operating room, in which tank it is reboiled. Provision is also made for furnishing hot and cold water from this tank for use in the operating room. The pipes with which these connections are made can be flushed by steam at any time when desired. A large steam sterilizer is used. Steam under pressure at 240° F. is provided for the sterilization of dressings, towels, and operating suits. There is also a boiler for the sterilization of instruments in the same department. The operating room, as we have said, was fitted up regardless of cost. It is certainly a very beautiful and expensive piece of work, but there is no doubt that just as good results could be attained with vastly less expenditure. At the same time as a tribute to modern surgery it is a beautiful thing, and as an advertisement of the hospital and its facilities it will doubtless prove a good investment.

SOCIETY PROCEEDINGS.

CALIFORNIA NORTHERN DISTRICT MEDICAL SOCIETY.

Semi-annual Meeting held at Red Bluff, March 10, 1896.

The sixth semi-annual meeting of the California Northern District Medical Society was held in Knights of Pythias hall, the Society being called to order by the PRESIDENT, A. W. HOISHOLT, of Stockton.

Addresses of Welcome.—J. S. CAMERON, of Red Bluff said: It is my pleasure, on behalf of the members of the local profession, to welcome you, members of the most progressive profession of this latter end of the nineteenth century, to Red Bluff. Divested of all ornament, our profession is devoted to the alleviation of human suffering, and I hope it will never be said that a member of the California Northern District Medical Society was ever found derelict in his duty to his profession, his country, or himself. I again welcome you and wish you God speed in your labors to-day and good cheer to-night.

JOHN FIFE, of Red Bluff, Chairman of the Committee of Arrangements, said: It may be news to some, that about ten years ago a medical society was organized in this town, which was given the name at present borne by this Society. Its territory comprised Colusa, Shasta, and Tehama counties, and its membership was about twenty. It flourished for a number of years, but at length died of inanition. There is no doubt that it did much good, if in no other way than in elevating the professional ethical spirit and suppressing the spirit of commercialism. This point, it seems to me, is of considerable importance and deserves more attention than is usually bestowed upon it by those who deliver addresses touching upon the benefits of medical organizations. There can be no doubt that the professional tone and consequently the respect in which the profession is held, is much higher in communities in which medical societies exist. Your Committee of Arrangements has used its best efforts to bring into this Society the members of the old society. I am afraid that our success has not been as great as we expected, but I hope that we have at least awakened them, and that those who fail to come in at this meeting, will do so at the next.

THE PRESIDENT, A. W. HOISHOLT, of Stockton, read an address [published at p. 177].

The Radical Cure of Hernia.—T. W. HUNTINGTON, of Sacramento, read a paper upon this subject. It was prefaced by a brief review of the more important features of hernial anatomy, and attention was directed to the fact that descriptions of the inguinal region are usually obscured by non-essential detail. The position maintained was that the term "Radical Cure of Hernia," is now acceptable to most modern authors. The methods described and recommended were those of Bassini and Halsted, and allusion was made to the dangers attendant upon the latter procedure from atrophy of the testis. Kocher's operation was also alluded to as possessing certain probable advantages. The necessity for a perfect suture material, and the advantages of kangaroo tendon as such, were dwelt upon, and allusion was made to the complications present in herniæ, involving the sigmoid flexure. In closing, the author alluded to the surgeon's personal responsibility in this field, and the requirements for absolute asepticity in all that pertains to it.

DR. J. R. LAINÉ, of Sacramento: I am very much gratified at the success that recent years have shown in this operation. As early as 1869, a gentleman at Washington, Ill., desired me to study the subject with a view to ascertaining, if possible, if there was some feasible way of effecting a radical cure without operation. I studied the text-books that were then regarded as foremost, and came to the conclusion that there was no practical means by which a radical cure of hernia could be obtained without cutting down. He urged, at that time, that if any one—unless in a case of strangulated hernia—were to advocate the operation of cutting through the skin, the operation would fall flat. I believe that I entertained that opinion until a few years ago, when the present operation was agitated. Since that time my opportunities for performing it have been limited. At the present time there can be no question that in many instances it is the only thing to do, especially if a man is compelled, by reason of his vocation, to work at manual labor. The author has not stated the length of time usually necessary to make a recovery so that the patient can walk about without any restraint. Those operations where I assisted seemed to have been successful, and I see no reason why I would not submit to such an operation, if I had reason to do so. I know that Dr. Huntington's operations have been painstaking and laborious. We on the coast are isolated more or less from other people, and yet, whenever we have occasion to, we operate with a boldness equal to those in metropolitan cities.

DR. H. D. LAWHEAD, of Woodland: I would like to ask if there was any difficulty from traction in drawing the conjoined tendon down and attaching it to Poupart's ligament; also whether there was any irritation in the floor of the new canal formed where the Marcy stitch was used. As I understood the author, there is what you might call a rigid line, about the center of the floor of the new canal. I should also like to know whether, in the very recent operations, there were any of those serious difficulties from hemorrhage from the arteries about the abdominal rings, of which the older books speak so much.

DR. J. M. WEST, of Red Bluff: I was very much impressed with the common sense with which Dr. Huntington's paper was impregnated, especially the doing away with the seven or eleven anatomical coverings, and going right down and reaching the sac at once. There is one point which I would have liked Dr. Huntington to have dwelt on more thoroughly, and yet he hardly alluded to it, and that is the cases in which he would advise a physician who felt competent to perform the operation to recommend it. Of course, in strangulated hernia, there is no question about it; but there are many cases in which the protrusion can be retained with an easily fitting truss. I would like to know when he would advise an operation and when he would advise the patient to go through life with the slight inconvenience from wearing a truss.

DR. THOS. ROSS, of Sacramento: I have witnessed a number of the author's operations. In every case that I have seen, there was little or no constitutional disturbance afterward. There was no fever, and the cases proceeded to a rapid and perfect recovery. I can hardly conceive how a relapse could occur in this operation, because you have three distinct barriers: (1) The floor or bed of the cord. (2) The attachment of the conjoined tendon to Poupart's ligament. (3) The fibers of the fasciæ of the external obilque and Poupart's ligament. On the last occasion when I saw the author operate, he supplied another barrier which he has not mentioned. He placed a continuous suture in the adipose tissue beneath the skin. There is little or no danger in the operation, and its results are so uniformly successful, that I think it is important in all cases to have this operation done.

THE PRESIDENT: I remember seeing Dr. Marcy at the American Medical Association demonstrate this material that Dr. Huntington speaks so favorably of—the kangaroo tendon. I would like to know whether it is expensive material, and whether it was introduced by Dr. Marcy?

DR. J. S. CAMERON, of Red Bluff, enquired whether the kangaroo tendon would do for the purpose of ligatures?

DR. HUNTINGTON, of Sacramento, in replying, said the kangaroo tendon makes the very best ligature in the world. The subject of hernia operations has interested me as deeply as any that I have ever undertaken. I have studied it tolerably closely for the past 6 or 7 years. I have at different times been of a different mind as to the propriety of it. I have now anchored myself; I believe, firmly, and fully expect to endorse the operation in the future as I am doing here to-day. I think that Sir James Y Simpson (my authority for this is Dr. Shiels, of San Francisco, and I believe he is correct in this matter), many years ago laid down this aphorism in surgery, that the hernial coverings are but two in number, and he described them as the tunica propria and the tunica adventitia—the tunica propria being the skin, and the tunica adventitia all that lay between the skin and the sac. It seems to me that for the purposes of the surgeon those distinctions are admirably made—brief, clear, and concise, and can be remembered. No one, then, will ever be in doubt about what he has to deal with when he comes to a case of hernia. He will remember that he has the skin and something beneath it. He will recognize the conjoined tendon and the intercolumnar fibers, because they are peculiar and unmistakable, their name is written upon them, and, after having the matter once in mind, as I said before to day, you will scarcely ever lose track of those two or three points.

In regard to complications during the operation and the dangers that were formerly laid down as staring the operator constantly in the face, they have been dissipated by modern efforts. In the first place we now make a large, full incision, giving the operator every opportunity to go down upon the parts carefully and intelligently, whereas formerly we went in—at least those whom I saw operate in my earlier days, when they did operations only for strangulation—very cautiously from a small incision and depended on the probe to loosen up a narrowed constriction and force the contents of the sac back into the abdominal cavity. There was danger in those days from hemorrhage from the epigastric artery. They placed it pretty well out of reach of the hernia knife, but sometimes the hernia knife would sever it, and in those days a hernia was a most formidable and most unfortunate occurrence. To day we have the wound laid freely open, and it is my practice before beginning my operation fairly—that is, before severing the intercolumnar fibers, as one of the early steps in the operation, to get my finger down and feel for the artery itself, and locate it, and then a very little care will enable one to keep away from it. I have never seen any difficulty in dealing with any of my cases in that respect. I have had only one ugly hemorrhage after a hernial operation, and it occurred from the wounding of the branch of the artery accompanying the cord. It bled so profusely that I was obliged to go in and undo the work which I had done, and do it all over again. That has happened but once with me, and I believe it will not occur again. In preventing such an accident I believe it is very important to keep your wound dry during the operation, and to see that every bleeding point is picked up immediately. You can accomplish two things in that direction: you stop the hemorrhage, and you avoid staining the tissues with the blood, and you have your raw tissue before your eye for inspection as you come to it. In regard to the time required for the treatment of these cases, I will say that there is a difference of opinion as to the advisability of letting the patient up in a comparatively short time. I find it extremely difficult to keep patients in bed after the third week. Even after the second week they begin to beg me to let them sit up, and I have to debate the question with them at some length, and insist that the tissues there are not in the proper condition to bear the weight of the abdominal viscera until a somewhat protracted period. I have let my patients walk about at the end of four weeks. In some cases they should not walk under eight or ten weeks. I believe, however, a period of four weeks, where the wound has healed, is practically long enough. With regard to the class of cases which one should operate on, I may say in most of the cases I have operated on in the past 5 years, the patients have come to me seeking the operation themselves. They have been largely laboring men, or office men, or people employed in such capacity that they have been brought in contact with others who have been operated on, and it is a source of considerable gratification to me that the patients themselves who have been operated on are the best guarantee for the operation. A man comes to me and says, "I know so-and so who was operated on for hernia, and I have been suffering from the same annoyance so many years, and I am sick and tired of it; in hot weather I perspire and the truss gets moist and smells badly, and it is a nuisance, and I am willing to devote a month's time and have the operation done." And I am always willing to do so. A long time ago I gave up advising people to submit to operations except where they were incompetent to judge for themselves, or under peculiar circumstances. The question whether an operation is to be performed should be left to the election of the patient and not the surgeon. I think the surgeon goes beyond his jurisdiction when he seeks out his patient, or when he approaches his patient with the idea that "you had better have that operation done, and done now." I am quite willing to say to those people, "I think you can be relieved and get fairly on your feet and able to go without a truss in a comparatively short time." But I give them no guarantee, of course, and give them no advice further than that I am willing to do the operation.

DR. B. M. GILL, of Dunsmuir: If you had a patient who could not wear a truss, and who was in constant misery, would it not be better to advise him to undergo an operation?

DR. HUNTINGTON: I should advise him that he could probably be relieved. I would not advise him to have the operation done directly. I am very positive upon that point. With regard to the expense of the kangaroo tendon, each ligature costs 10 cents—$10 a hundred—and while that seems like a large amount, yet it does not cut much figure in this operation. For instance, where you do the Marcy stitch, you do not need more than two or three tendons. I have, however, found the burden of expense somewhat considerable, because I have come to use the tendon in many other ways. I use it for muscle suture in amputations where I wish to suture the muscle over the end of the bone to keep it from retracting. I use it in closing the operative wound for appendicitis and other abdominal wounds, and, in fact, in a great many ways Where I formerly used silk or catgut, I find the tendon very available. I want to reiterate the absolute necessity for cleanliness in this work. To attempt to do this operation without a full knowledge of all the requirements in that connection is simply to invite the most utter humiliation and defeat. I cannot conceive of any more embarrassing position for a man to be placed in than to take a patient who is going about his business, comparatively comfortable, wearing a truss, able to earn his living, and put him to bed to remain for a period of months from suppurating sinuses caused probably by some fault in the operator's personal cleanliness, or in the *technique* which he has employed. I believe the success that has been reached by the advocates of this measure forms the best monument to the name of Sir Joseph Lister that has ever been erected to it.

DR. A. T. HUDSON, of Stockton: Have you had much experience with silkworm gut?

DR. HUNTINGTON: Yes; I have had great experience with it, and like it very much in certain localities. I do not like it as a buried stitch. It never gets soft. It is very liable to leave a pricking sensation underneath the skin, and in a number of instances where it has been used it has been found necessary to remove it. It is a very available suture material, and it is to be regretted that there is that one objection to it; otherwise, it seems to me, it would be even more ideal than that which is advocated by Marcy. For a skin suture it has come to stay, unquestionably. I believe it to be, under all circumstances, the very best thing we have for that purpose.

Items of an Experience of Forty Years with Chloroform.—A. T. HUDSON, of Stockton, read a paper upon this subject [published at p. 205.]

DR. J. R. LAINÉ, of Sacramento: I confess to having a predilection for the use of chloroform. In my younger days I saw Professor Gunn, at that time of Rush Medical College, Chicago, have chloroform administered during the entire winter, and on no occasion did I observe any ill effects from it. At that time it was the custom, I believe, for him and some other surgeons in Chicago, to use it almost exclusively. There were classes of cases, however, in which we were enjoined not to administer chloroform, for instance, chronic bronchitis, or other organic diseases of the respiratory organs, associated with cardiac disease. We, at that time, thought it better to use it only as an experiment, and as soon as we discovered that there was some ill effect from it, to use something else. We take the same course at the present time. When we find that one anesthetic does not act well, we generally use another. With reference to the anesthetic in abdominal section, we know very well that the inhalation of ether will excite some irritation of the bronchial tubes, and induce coughing. It is rather a serious matter when the abdomen is opened and the intestines apt to protrude, or when you are removing a cyst, to have the patient coughing. This protrudes the contents of the abdomen through the external wound and you have to stop your operation. We know that chloroform will not produce as much bronchial irritation as ether. It is a question merely as to your belief in the safety of its use. I have observed that the man who is accustomed to administering ether is not a good man to administer chloroform, unless he is accustomed to the use of the one as well as the other The methods of application of the anesthetics are different. The rule a long time ago in administering chloroform was, let there be as large a quantity of atmospheric air as possible; whereas, we use ether in as concentrated a form as possible. With modern appliances, where you can only apply a few drops on the inhaler at one time, it is much safer than to use the cone, where you have to pour in probably a drachm or two drachms of chloroform. I agree with Dr. Hudson in regard to the benefit to be derived from a hypodermic injection of morphine before the operation. The anesthesia from chloroform is not as profound as full anesthesia from ether. The reflexes are not overcome as soon. You need something, perhaps, to use as an adjunct, and also to relieve pain after the operation. For that reason I am a believer, and most generally use, a hypodermic injection of morphine before the anesthetic. I think it is a good thing to use the alcoholic stimulant before the operation; it frequently sustains the heart. Those people whose sensibilities are injured by using an alcoholic stimulant can use oil of turpentine with the same effect.

Dr. C. A. Ruggles, of Stockton: While listening to the paper of Dr. Hudson my mind went back fifty years to the first *capital* operation that was ever performed under the influence of sulphuric ether. On November 6, 1846, at the Massachusetts General Hospital, I saw the amputation of a thigh by Dr. Hayward, the first that ever was done in the world. Dr. Warren stood there assisting. I have, therefore, a little interest in the discussion. Brought up, as I was, in Massachusetts, it is not strange that my sympathy should be with ether.

Dr. J. S. Cameron, of Red Bluff: The most masterly article that has ever been written on the use of chloroform was one by Dr. Chisholm, of Baltimore, some fifteen or twenty years ago. He gives a record of chloroform administration 118,000 times without an accident. He says that our sheet anchor in heart disease in performing a surgical operation is chloroform; the shock delivered to the system by the operation is much severer than any shock that chloroform can produce. I have administered sulphuric ether a good deal, and the more I give ether the less I want to give it. The discovery of the administration of ether properly belongs to old Dr. Young, of Georgia. He administered it three or four years before Morton attempted to get a patent on the administration of ether. The State of Georgia has put Young's picture in the United States Capitol in honor of that fact. He was the first man who ever gave sulphuric ether.

Dr. Thomas Ross, of Sacramento: I have used and seen chloroform used, very much more frequently than ether, and I have never seen any bad accident occur from chloroform. Several times I have seen patients get pretty close to the stepping off place, but they fortunately rallied and recovered. I think, with Dr. Hudson, that the principal thing to remember, is not to overwhelm the patient at the beginning with the chloroform vapor. I think care in the first place would prevent some or many of the deaths that are recorded, as most of them occur in the first stage of anesthesia. Chloroform certainly has the advantage of not producing the intense bronchial irritation that ether does, and vomiting, which frequently destroys the effect of operations, especially abdominal operations, seldom occurs. I like chloroform, so far as my own individual experience is concerned. Still, in reading the journals and papers, we so often come across accounts of deaths from the use of chloroform, that in order to be on the safe side, in any case that I am personally interested in, I generally prefer ether. The injurious effects of chloroform generally occur in persons of plethoric habits.

Dr. J. H. Parkinson, of Sacramento: I am, like Dr. Ruggles, rather in favor of ether. In the first place, I do not think there is any doubt that it is easier to give chloroform than ether. More people can give chloroform than can administer ether properly. Much less apparatus is required with the former, because, as Dr. Lainé has said, the basal principles of the administration of the two drugs are diametrically opposed. Chloroform should be given largely diluted; ether should be given in great concentration. In order to attain the latter result, you must have an efficient apparatus. To administer chloroform you require very little apparatus, or only such as can be constructed in a very short time. The inhaler that Dr. Hudson has exhibited, which is identical in principle with the Esmarch that I use, is as convenient as any. Skinner's old inhaler, was a piece of material stretched over a very simple wire frame, through which air, impregnated with chloroform vapor, could be inhaled. An inhaler is very easily obtained, and if the point be borne in mind that chloroform should be given largely diluted, and, which is probably more important, only drop by drop, then you have an efficient means of administering chloroform always at hand. I therefore say it is easier to give it. I am not biased in favor of either drug. I use chloroform with children, I use it in obstetrical practice, I use it with young people when I have occasion to do so, but I contend that ether is the safer anesthetic. It is also a perfectly practicable one. There is less danger attached to it than there is to the administration of chloroform.

There are fewer fatalities, and with the exception of two drawbacks, one of which Dr. Hudson has mentioned, there is very little to be said against it. The principal danger of ether, is in renal complications. There is no doubt that if albumin be present in the urine, it is a dangerous anesthetic. There is also no doubt that in elderly people, or in the presence of any bronchial complication, ether is a more dangerous anesthetic than chloroform. As regards vomiting, my experience with ether has been large; my experience with chloroform less so; but I have seen chloroform administered by others a number of times, and I have administered it myself a number of times, and I have not, from that experience, which dates from 1875, made up my mind which is most often followed by vomiting. Quite frequently after chloroform, I have seen vomiting. I think the vomiting depends largely on the condition of the stomach; a patient who has eaten a meal recently, will probably vomit from any anesthetic. If you prepare your patient properly beforehand, you will probably not have it in either case. The feeling of suffocation depends largely upon the administrator. I have taken ether myself; I know what it feels like. I have also administered it a great many times, for a number of years. I believe if it is given properly, there will be very little suffocation. I have given ether when four or five students have been holding the patient on the table. That is absolutely wrong. As a rule, if it be necessary to hold anybody down when you are administering ether, you are giving it the wrong way. I admit the patient may have to be held sometimes, but this is during the second stage of anesthesia, and this is common to both ether and chloroform. As to the time necessary to secure anesthesia, my experience has been that anesthesia by ether is rapidly accomplished. A long time ago I used to keep a record for another person on this subject. I think two minutes was the average time. This cannot be done in all cases with a pasteboard cone, nor with a newspaper, nor napkin, but it can be done with an efficient apparatus. I have seen it done many times. I have never seen a case in which it was not possible to produce complete or profound anesthesia with ether. As to the relative safety of ether and chloroform, I concede what has been said by Dr. Hudson, and by other speakers, and in print over and over again, that there are deaths charged to chloroform which ought not to be charged to the anesthetic—patients who would have died anyway, from shock or other causes. If you are going to take statistics, you must take the reports of all the cases available. I do not think the statistics of the one anesthetic are any more reliable than of the other, but taking them for what they are worth, we have one definite and positive fact, and that is that more deaths occur from chloroform than from ether. It is said by the advocates of chloroform, that this is so because more administrations of chloroform have taken place and are taking place in the world, than of ether. I do not deny that there may have been, from the first use of anesthetics, more administrations of chloroform, but I do say, as a matter of fact, we have had more deaths. I believe, most firmly, that if all the cases in which chloroform has been used, had been anesthetized by ether, we would not have had all those deaths. We have the one absolute, positive fact, that under chloroform patients will die, and they die in a mysterious, peculiar, and, to me, very distressing and appalling manner. They die very rapidly and give you very little warning. That is something that never takes place from ether. I do not know that I have ever seen recorded a sudden death from ether. A hospital is a great ægis for various things, but to a man in country practice, particularly, it is very much more consolatory for him, to have the patient die two or three hours after the operation, or on the following day, than on the table, because everybody in town knows that he died as the result of the anesthetic, when he dies on the table; at least, it is presumed so. If he dies on the following day, or even four or five hours afterward, the death may have been due to something else. To me, that is a comforting reflection. I may know the patient died as a result of the anesthetic, but I do not think the public will know it, and the moral effect on the people who will be operated on in the future is much better in that particular. When I am giving chloroform to a patient, and he begins to act badly, I begin to feel uncomfortable myself, because I don't know whether that man is going to die, then and there, and nobody knows. He will die in spite of all you can do to prevent it. On the contrary, if my patient under ether begins to act badly, I feel solicitous, and that is all. Of the various inhalers on the market for giving ether, I prefer the Ormsby inhaler. It is one that I have assisted in perfecting, and I believe in it simply because it is efficient. It is portable, readily cleansed, and the parts are readily replaceable, and is not bulky. In regard to the dilution of chloroform with alcohol, or as in the A. C. E. mixture, I think that is playing with the subject. The danger of administering these mixtures remains in the exact quantity of chloroform you are giving.

Dr. T. W. Huntington, of Sacramento: Like Dr. Ruggles, I was educated in an atmosphere where chloroform was not breathed. I was taught that it was next thing to criminal to use chloroform as an anesthetic. I fully believed that for a long time, and I must say I have not gotten over the prejudice which I imbibed there against chloroform. I realize with Dr. Parkinson that there are a class of cases that die very quickly. They absolutely go dead, and you cannot bring them back to life under any circumstances nor by any means. It requires but a few drops of chloroform to effect this. It has been recorded time and time again that a patient has been placed upon the table, a handkerchief, cloth, or inhaler put over his face, he has inhaled a half-dozen or dozen breaths of chloroform vapor, and he has simply been snuffed out; he has gone. That is one thing, as Dr. Parkinson has said, that is against the use of chloroform as compared with that of ether. Chloroform is very much safer in experienced and skilled hands than it is in unskilled hands, and that is a very serious and to many people a positive bar to its use at all. I believe that chloroform in unskilled hands should be prohibited altogether. I believe it is a very dangerous agent in unskilled hands. Take the circumstances of any of us when we proceed to any undertaking requiring an anesthetic. We cannot stop and give the chloroform ourselves; we cannot be distracted, annoyed, and harassed by the little incidents that attend the administration of an anesthetic, and usually it is turned over to the youngest man in the party, or to a nurse, or assistant, or somebody who cannot be regarded as experienced. For that reason, taking the facts in the case, it seems to me that we are practically tabooed from the use of chloroform. I would not hesitate to use chloroform if I could give it myself, or if I could superintend the giving of it, but it is impossible for me to do that. I should be losing at one point where I would be making a gain at another, and I do not choose to do it. On the other hand, what are the conditions? You give your ether to a person who is fairly experienced—to a person who has seen it given a sufficient number of times, and who has imbibed sufficient instruction to give it fairly intelligently, and you can leave him alone and your patient will not suffer. He may not give it quite as skillfully as I or somebody else would give it, but he will give it, and your patient will live through the operation and come back to life afterwards, and that is a very big point. If I could feel that I had a nurse who had seen ether given a half-dozen times under my instruction, or under the instruction of any person experienced in it, I would be perfectly willing to take those chances. I certainly would not take them with chloroform, and I should be making a very grave error if I did. In regard to the operator's own piece of mind under one anesthetic or the other, admitting that you have a fairly experienced anesthetizer, and that you are giving chloroform, I believe no operator can proceed to devote his whole time and attention to the work before him so well when his patient is taking chloroform as he can when he is taking ether. It has been my experience, and it is the recorded experience of other men, that you proceed to your work with your patient under ether with the feeling that you can abandon all thought of it, and go on with the work before you devoting your whole energies to it. I think it is a point which is well taken. The necessity for apparatus has been alluded to. I have used the modified Ormsby inhaler, which has been perfected by Dr. Parkinson, and I regard it as the best ether inhaler there is, but I am by no means at a loss if I have not it at hand. Recently I have been deprived of its use, and I find that I can get along very well with the old Bellevue Hospital inhaler, which is nothing more than a piece of paper put between two pieces of cloth and sewed together at each end. There is no patient living who can be anesthetized with anything who cannot be put soundly and surgically asleep under ether. I am willing to make a very strong assertion on that point. I have seen it given a great many times. I have given it a few times myself; I say a few times. I hear men constantly saying, "I have given chloroform hundreds of times." Do not place too much confidence in that. The man who gives chloroform a hundred times in the course of his lifetime is an exception to the average run of family doctors. I will except from that statement the chloroform given at child-bed. I know how many times I have given it in 20 years, and I will astonish you if I tell you. I have given chloroform during the last 7 years of my life less than 100 times a year, and I have had a pretty active hospital practice, and a fairly large outside surgical practice. I have heard men say they have done hundreds of certain operations, when I knew that it was impossible in the ordinary course of events. I have given ether a few times. I have seen it given quite a good many times, and I have never but once been obliged to resuscitate a patient, when I thought he was absolutely in distress. I have a few times drawn a patient's tongue out of his mouth. I do not think it will exceed a dozen times. I have not seen anybody give chloroform but that he was going to the patient's head very often, and a fair proportion of the times the operator has been obliged to stop the operation and do something to bring the patient back to life. I know something about what the distress and humiliation of a death is, for I happened to have had one myself under it. I have not given chloroform to exceed 25 times in my whole experience, and I have killed one patient. I do not believe that I will give chloroform to a patient again when I will take any chances on killing him. In this case the anesthetic used was the A. C. E. mixture, and I think it was poured upon the cone a little too freely. However, the entire amount of anesthetic used was only 2 ounces, so that I believe he only took a half an ounce of chloroform. It was administered by an attendant who had been giving anesthetics a long time. He had used the A. C. E. mixture before, and it was given on an ordinary cone, I believe quite as skillfully as it should be given. I am well satisfied that the number of deaths from chloroform far exceeds those from ether. Regarding cases of chloroform fatality on this coast, I know personally of one of them. Dr. Gross, an able and skillful surgeon of Eureka, Cal., and one who has had long experience, had a patient come into his place one afternoon. He was not at the time drunk or under the influence of liquor, but he was a man who occasionally took a drink. He came in to have a dislocation of the shoulder-joint reduced. He said he wanted to take chloroform, and added, "Let us go down and take a drink," and they stepped out and each took a drink of whisky. Upon returning, the man was placed on the table. The doctor used a handkerchief and poured chloroform on it once, and not to exceed 1 drachm at that time; he laid it over the man's face, and he died almost immediately.

Dr. Parkinson, of Sacramento: A case happened in Sacramento several years ago. I think the operation was for fistula in ano. That patient died almost immediately. A case was reported not long ago in Oakland.

Dr. Lainé, of Sacramento: How about the two cases which Dr. Parkinson spoke of as dying from ether?

Dr. Parkinson: In both those cases the deaths occurred some time after the operation; not on the table. That is the point that I have raised. I want the patient to live through the operation every time, and I think every gentleman here will say the same thing. I admit it does not make any difference to the patient, but it makes a whole lot of difference to the doctor.

Dr. Ross, of Sacramento: We all like to see a man live some time after the operation. I wish to substantiate what I have said as regards the frequency of administration of chloroform. I have never given anything else than chloroform in twenty-five years, giving it for everything. I always use it in my midwifery cases, and in those cases, of course, you do not produce complete narcosis, but you use your chloroform in the stage of narcosis that is generally considered the most dangerous. The man who does not use chloroform more than three or four times a year has very little to do. I am satisfied that I have given and seen chloroform given as an anesthetic in hospitals, and in private practice, hundreds of times.

Dr. Huntington: I said the average family practitioner. There are a great many practitioners who never use chloroform or ether in a lifetime.

Dr. Laine: The manner of stating the case is hardly a fair one. I have heard it said that more cases die from chloroform than from ether. It is an unfair way to make the statement. It should be that there is a greater percentage. In stating a percentage the statement must be based upon statistics that have been tabulated, and, in the absence of any such statement, we must merely take it as the individual opinion of the person who states it.

Dr. B. M. Gill, of Dunsmuir: Several States have enacted laws that no surgeon or physician can administer chloroform and perform an operation alone; he must have another surgeon or physician, or skilled assistant, with him. That being the case, it brings up the medico-legal question as to the advisability of administering chloroform, and the question arises in my mind, if a great deal of caution is not used, whether a man would be liable to a suit for malpractice, and how far these statistics would go in such a case. Not many years ago I heard of a suit in the city of Philadelphia in a case where a dentist administered an anesthetic without a skilled assistant, and the patient died.

Dr. J. M. West, of Red Bluff: This question of safety with an anesthetic depends largely upon who is giving it, and whether he understands what he is giving. I use chloroform ten times where I use ether once. Of course, if I have a case which is a bad one, or a prolonged operation, I want ether, but I want a man to administer it who is accustomed to handling ether. I would rather give the chloroform and do the operation alone, than have a man give ether who was not accustomed to giving it as an anesthetic. I have only had to resuscitate one case from the effects of chloroform. In 1860 all the operations I witnessed under anesthetics were under chloroform. During the first eighteen months of my service in the army I administered chloroform 250 times. I had never seen ether given up to that time. I became convinced that chloroform was the thing, and continued to use it after I left the service. Some twelve years later than that, a good many of those gentlemen in the East, like Drs. Ruggles and Huntington, introduced the idea in Illinois that ether was the safer anesthetic. The first case that I witnessed was one in which Professor Holmes, of Chicago, performed an operation on a little, low, heavy-set fellow that I knew very well. He put him on a table and administered ether; turning around to us he began telling us how he was going to make his operation. In a moment he found the patient had ceased to breathe, and had passed over to that country from whence no traveler returns. That did not make a very favorable impression on me, but I have given ether a number of times since, and if I have a protracted case I want ether. I believe, in the majority of cases, if we had a man who understood handling the anesthetic, we would have very few deaths from ether. I have known of two cases where death occurred; where vomiting had taken place, and an examination afterwards showed that a small quantity of food had passed into the trachea, death being produced by that mishap. I believe if I was to have a patient die on the table I would want him to die under the influence of ether, because the majority of the profession is in favor of it. I am like Dr. Parkinson, I want to see the patient get off the table, even if he die twenty minutes afterwards.

Dr. H. D. Lawhead, of Woodland: I wanted to ask one question, and that was in reference to the wholesale condemnation of the use of the A. C. E. mixture. We should grant the same conditions that we would grant with the use of plain ether or plain chloroform; that is, that the article used must be genuine. I remember a few times when a patient behaved badly under chloroform and under ether, that the same patient responded more favorably to the A. C. E. mixture. I have seen it used a great many times, and the results were very satisfactory; the patients were less obstreperous than under either of the other anesthetics. If we condemn it, there must be some reason for doing so. Now, what are the physiological reasons for the condemnation of it, provided our patients are properly prepared beforehand by stimulation and a hypodermic injection of morphine and atropine.

The President, Dr. Hoisholt, of Stockton: Dr. Ruggles and Dr. Huntington lived in an atmosphere, so to speak, of ether, and saw others live through it. I lived four years in Germany, where everybody uses chloroform. I was attending once on Prof. Czerny's clinic, in Heidelberg, who was operating all the time, and he used pure chloroform. I forget now, whether it was one or two fatalities that he had seen, but I think he could truthfully say he had administered it thousands of times. Throughout Germany to-day, chloroform is the anesthetic used, and when we hear that and also hear the other side, it would occur, perhaps, that the danger of chloroform over that of ether is but slight, provided it is used by experienced men. In Germany, they never allow an assistant or a layman to administer chloroform. It is always done by a man who is not only a physician, but skilled in administering chloroform, and that is probably one reason why they do not see these bad results. Perhaps the danger in this country arises from the fact that the anesthetic is so often administered by men who are not fully qualified. In such a case, I fully agree that the administration of chloroform is criminal, while ether can be given by assistants who are laymen.

DR. CAMERON, of Red Bluff: Dr. Chisholm gives a record of 18,000 administrations of chloroform, without an accident. It was given in Stonewall Jackson's corps, during the war, 16,000 times, without an accident. We had no sulphuric ether at all down there during the war; we used chloroform entirely.

DR. HUDSON, of Stockton, in replying, said: There are quite a number of deaths reported as having occurred in the Southern army, but the exact figures I do not know. My experience is gathered largely from my own use of chloroform. I have taken chloroform a great many times, and ether a few times, and I have taken chloroform and ether mixed. The breath will be offensive from ether for from 6 to 12 hours, while the effect of chloroform is soon gone. I have come to the conclusion that if you dilute chloroform somewhat you overcome the danger of immediate mischief, and if you give it gently you have got as good an anesthetic as can be found. One thing against ether, is the difficulty in holding a patient perfectly still, so that the surgeon can feel that he is not disturbed, and that there is nothing to annoy him. With chloroform, you can hold your patient perfectly quiet through an operation of three-quarters of an hour or an hour, without any trouble. I have never seen this done with ether. I have given it several times, and I have given half a pound at a time without accomplishing my object—that is, I had to finish up with chloroform.

AFTERNOON SESSION.

Anomalous Disease of Knee-joint.—A. P. TARTER, of Tehama, reported the following case, and exhibited the patient: J. B——, aged 30; married; family and personal history good. About one year ago, received an injury to his left knee, from a slip while pushing a loaded car in a warehouse. His trouble began about this time, and was initiated by occasional pains close to the outer side of the insertion of the ligamentum patellæ. These attacks would occur at longer or shorter intervals, and though the pain, at times, was quite severe, he kept at work, and the annoyance would cease after a few days. He continued to be annoyed in this manner, and about November, 1895, after an interval of freedom, when experiencing a return of the pain, he noticed a swelling in the vicinity of the ligamentum patellæ. He continued to work, but was compelled to quit, November 23, because the pain had become too intense. He consulted the speaker on the 29th, who found a very tense swelling, which extended laterally for an inch on each side of the tendon, and for about half way up the patella. The contents were, apparently, fluid. He had not slept well for three nights, because of pain. A blister was applied over the entire surface of the tumor, and a posterior splint with compression, patient being kept in bed. Very little if any change had occurred in the tumor, though the pain had departed and he rested well at night, by December 4. On this date, under antiseptic precautions, an effort was made to aspirate the tumor, using a moderately fine needle, and inserting it on the outer side, a little above the insertion of the ligamentum patellæ. The speaker failed to get fluid, and on removing the needle, it was seen to be filled with very heavy clear liquid, containing particles resembling boiled sago. These latter had obstructed the lumen of the needle. A swelling had appeared at inner side of knee opposite the upper edge of the patella about the size of a robin's egg. This was quite soft, and soon increased to the size of a hen's egg. It could be dispersed on pressure, to return slowly when pressure was removed. This pressure gives patient a sensation of fluid flowing beneath the patella toward the tumor, which appears to be a little less tense. There was no swelling of the knee-joint proper at the first examination. A few days following the attempted aspiration, a small soft tumor developed on the inner side of the knee-joint, on the opposite side to where the puncture was made. The physical condition of the patient has always been good. At one time his febrile symptoms were quite pronounced. His pulse has not been below 90 for a long time, and was 102 quite recently. His temperature was taken regnlarly, and there appeared to be no rise at night.

DR. J. R. LAINE, of Sacramento: I believe the trouble is masked by reason of the compression it has received by splint. I base my opinion almost exclusively, aside from appearance of the knee, on the statement of the attending physician. He states that when he pressed his aspirating needle into the tumor and withdrew it, there was attached in the eye a sago-like particle. That would indicate the large cells characteristic of sarcoma. I believe it to be a sarcoma.

Dr. T. B. Reardan, of Oroville: I should believe this to be tubercular trouble, taking the history of the case and the temperature.

Dr. Thos. Ross, of Sacramento: It is a case of tuberculous trouble. I come to that conclusion partly from the history of the case and partly from the appearance of the individual.

Dr. B. M. Gill: Before I would venture to give an opinion I would like to know the opinion of a microscopist. I think the microscope would reveal this thing.

Dr. T. W. Huntington, of Sacramento: Dr. Lainé has raised the question of malignancy here. In my experience, the form of malignant trouble which he has suggested does not ordinarily originate in the joint itself. Sarcoma affecting the tendon would be a very rare thing. It does occur, but the rarity of it would pretty nearly exclude it. I know of but one English author who speaks at any length upon that subject, and that is Senn. He has called attention to it and described the disease very carefully. He has also described the form of malignancy which Dr. Lainé speaks of. I will not be certain that Senn admits the presence of sarcoma of tendons and of joints. It certainly is, in my opinion, a very rare complication. There is another feature of the case which would, to me, practically bar that question, and that is the cessation of symptoms, absence of pain, or absence or lowering of temperature. The temperature recently dropped, and I should say that would be against the theory of malignancy. Sarcoma affecting the bones, and, I presume, affecting any other part of the human system, while it may be slowly progressive, is progressive. In my experience, I have never seen a rescession such as we apparently have here—a period of quiescence such as we are having here now. On the other hand, on the theory of tuberculous trouble, you could certainly have this condition. I am strongly disposed to think this trouble is of tuberculous origin. It is a very interesting and anomalous case, one such as few of us will ever see again. I should proceed upon that theory, and insist upon long immobilization, and hope that permanent ankylosis would take place. If that does not occur, and this condition should remain, I should certainly advise immediate resection of the joint. I think the man would bear a resection and would go out of the surgeon's hands in fairly good condition. If I immobilized the joint, I should put it up in the form of partial flexion, as it is now.

Dr. J. S. Cameron, of Red Bluff: About twenty years ago I had a case almost similar to this, except that there was a little more fluid in the joint. I treated him for a year and could do nothing with it. I then sent him down to Dr. Toland, who put an apparatus on the knee, and in six months it was well, and remained so until the patient's death. The joint was completely ankylosed.

Dr. J. M. West, of Red Bluff: If my memory serves me correctly, Senn says that it is not necessary to have a history of tubercular deposition to have tubercular joints. While there is no history of any specific disease in the family, I am very strongly inclined to accept the theory of tubercular trouble in this case.

The President: I understand Dr. Laine to say this might be a round-cell sarcoma, and that, of all forms of sarcoma, is the most acute in its course, and, for that reason, I should not expect to see it follow the course it has taken.

A Recent Case of Puerperal Sepsis, with a Local Diphtheritic Deposit.—B. M. Gill, of Dunsmuir, read a short paper embodying a report of a case of puerperal sepsis. Patient, a multiparæ, was delivered, on February 6th, of an apparently healthy child, after a normal and easy labor. On the evening of the day of delivery, the child had twice been put to the breast, and on each occasion had vomited quite badly. The following morning the child had a temperature of 105°, and was apparently suffering greatly. It died shortly afterward, having all the appearances common to patency of the foramen ovale. At this time the mother's temperature was 102°. Thirty-six hours later her temperature was 105°. The uterus was washed out with a 1:1000 bichloride solution, after which the temperature dropped to 103°. On the third day, on washing out the uterus, some cheesy fragments were noticed, and after gently using the dull curette, a quantity of the same material was removed. On the fourth day, the os was completely covered with a distinct diphtheritic membrane, the vagina and vulva being quite purple in color. On the fifth day the vagina was covered. Antipyretics seemed to have no effect upon the temperature, which, however, did not run above 104°. At this date, patient is rapidly improving, with every indication for a complete recovery.

Dr. H. D. Lawhead, of Woodland: The first question would be whether the doctor had been treating any septic cases, or cases of diphtheria, and what the constitutional condition of the mother and the surroundings had been. In the valley there has been, this season, an unusual number of septic cases—septic fever. It is almost an epidemic. I should have been afraid to employ, what I consider the heroic treatment the doctor used, a 1:1000 bichloride solution as an intrauterine injection. Fortunately, it appears that the patient did not suffer from that especially. It evidently must have had a wholesome effect. The doctor is to be congratulated upon saving the woman's life. In such cases I have thought that the best results were obtained from the use of peroxide of hydrogen (Marchand's) injections, and tamponing, not too tightly, with iodoform gauze, changing the dressing frequently, and stimulating treatment, as heroically as possible.

Dr. Thomas Ross, of Sacramento: I would ask if there had been any case of diphtheria in the neighborhood, or in the house at any time previous? It is very often difficult to detect the source of the contagion, but I think this was a case of diphtheritic sepsis with a diphtheritic membrane, but where it came from is hard to determine.

Dr. J. H. Parkinson, of Sacramento: In a case of this sort, without an absolute history of contagion, or a microscopical examination, it is impossible to state what was the cause. In septic cases we do find membranes that, of course, are not true diphtheritic membranes; that is, they are not positively diphtheritic in character. I do not feel warranted in expressing an opinion as to the identity of the case on that history. In regard to treatment, it has always seemed to me that curetting was something to be done very carefully. It is practically impossible with the curette to clean the *post-partum* uterus; that is, as one can do with a uterus which is not so far advanced in pregnancy. If the attendant is certain that any large piece of material remains that is decomposing, then, of course, he may debate the advisability of removing it, but to go over the entire surface of the uterus, and to clean it with a curette, would be a very difficult matter. Then, again, if the uterus is already in a foul condition, to expose the surface to any great extent, without cleansing it thoroughly and putting it in very good condition, is, I think, to expose the patient to rather greater risk. I think the use of the curette, after labor, had generally better be avoided. Some people believe in curetting very thoroughly, but it seems to me, when you review the whole case, in ordinary practice, it is better to leave it alone.

Dr. Gill, of Dunsmuir, in replying, said: The main idea I had in presenting this paper, was to bring out the reasons for, or cause of the diphtheritic membrane. I had not seen a case of diphtheria for some time; in fact, there was not a case in the neighborhood. The membrane did not separate, leaving a bleeding surface; it dissolved or sloughed away. There was no abdominal tenderness at any time. The curette used was a dull one, which would not wound the surface of the uterus whatever. I curetted because I discovered some peculiar cheesy pieces, about ⅛ inch square, which led me to think there was something behind, which should come away. In most of these cases of sepsis, I think we are too apt to localize the trouble entirely in the uterus. Undoubtedly there was more or less septic infection, absorption, etc., from decomposing material, but in this case there was also evidence of a general poisoning, possibly uremic, possibly due to absorption from the bowel. I believe that many people overlook, in their treatment of these cases, some of the general constitutional conditions. I am certain that some of the authorities that used to teach us to lock up the bowels with opium, were wrong. I think we should get every possible organ in order; we should get every bit of waste out of the system, by every channel that we possibly can. I believe, that in this case, had I not thoroughly eliminated by the bowels, the woman would have died. How much the febrile symptoms were due to these side issues I cannot say, but I do know that immediately on washing out the uterus and curetting, the heart's action came down so wonderfully that it led me to think that there was the principal point of infection. The pulse was at that rate, and rapidly increasing, running into one continual current, which many men have seen, and we know that in a short time, such a patient will be beyond help. Immediately after this treatment, the pulse rate was reduced to 108 almost in two hours, and the temperature to 103°. So that I felt that I had accomplished something. The odor from the uterus was bad, but not as bad as I have seen. This subject may not be new, but it is very interesting to many on account of the number of cases that Dr. Lawhead speaks of as having occurred in different portions of the State.

THE PRESIDENT: If I understood Dr. Gill correctly, he said there were some symptoms of uremia. If there was real uremia present, or if there was nephritis present in the case, then it was likely to have been diphtheritic, as that complication would be quite common in diphtheria.

Mental Therapeutics.—M. W. WARD, of Woodland, read a paper upon this subject. He said the influence of mental states upon bodily functions was evident from daily examples. It was shown in the stimulation or inhibition of the secretions of the various glands, and in the increase or diminution of the functional activity of the organs. Expectant attention would often work wonders. John Hunter had said he was confident he could fix his attention on any part of his body and produce a sensation in that part. Johannes Müller expresses himself in an equally positive manner. He says: "It may be stated as a general fact, that any state of the body which is conceived to be approaching and which is expected with confidence, a certain bodily occurrence will be very prone to ensue as a result of that idea." In the hypnotic state, the actions of the subject were in accordance with the ideas suggested by the operator. In some cases this persisted more or less strongly after the *séance*. Knowing, as we all do, the undoubted influence of varying mental states upon the bodily functions, the speaker thought it strange that more attention was not paid to the psychic element in therapeutics. That imagination and faith can exert an influence over disease, no one can doubt. The question was, what is the extent of this influence, and what are its limitations? Many persons are now being successfully treated by the following methods: (1) Prayer and religious faith. (2) Mind cure. (3) Christian science. (4) Spiritualism. (5) Mesmerism. (6) Suggestive hypnotism. Here are these different methods based upon widely different theories, and having but one thing in common, that they all cure disease. The obvious conclusion is that there must be some underlying principle common to them all. Paracelsus touched the keynote when he says, "whether the object of your faith be real or false, you will obtain the same effects." Continuing, the speaker enumerated the various methods by which the physician, who had obtained the confidence of his patient, could ameliorate mental and physical conditions, through suggestion and mental direction, coupled with judicious medication.

DR. B. M. GILL, of Dunsmuir: This paper has opened my eyes to one fact. It has taught me why the homeopaths sometimes succeed. Infinitesimal doses and a great deal of faith will undoubtedly accomplish a great deal. I confess to some little disappointment in the paper. I had hoped that the writer, in speaking of mental therapy, would refer to the application of various remedies that are known, to abnormal or pathological conditions. I realize, that in every case where there is an abnormal or pathological condition, there is always some underlying cause; that each case should be studied by the physician, whether he is practising in the country or in the city, whether he has a large practice or a small practice, to ascertain, if possible, what pathological conditions exist; whether they are of recent origin, whether they are inherited, whether they are acquired by some vicious habit or due to surroundings. There are many cases of mental aberration, both in the sick and in those who are comparatively well, that commend themselves to the study of every medical man, if they are brought to his notice.

DR. THOS. ROSS, of Sacramento: Faith is a good thing. We are told that faith will move mountains, but we have never seen a mountain moved by it. In a diseased condition, where you have organic disease—pathological changes in an organ—faith does very little, but in functional derangements, I think faith or confidence will frequently produce an ease and a tranquillity of mind that places the patient in the best condition to recover. As I take it, mental therapeutics, suggestion, and hypnotism, are all about the same thing. We know that healthy men of intelligence are not easily hypnotized; they are not hypnotized at all except by frequent attempts. I think that a hypnotic subject is in a diseased condition. The persons that are the most readily hypnotized are weakly, hysterical persons, persons of a neurotic family. I do not think that persons in a healthy condition, or strong, intelligent men, can be affected by mental therapeutics or hypnotism at all.

Dr. T. W. Huntington, of Sacramento: I dislike very much to criticize a paper, in the absence of its author, but my opinion upon these questions is pretty strongly fixed. I have never known any good to come out of the sort of discussion which has been aroused here. I have never known any good to come out of that line of research, if you please to dignify it by that term, and I am perfectly willing to go on record as saying that I believe that no such expression should leave this Society without having something said in disapproval of it. I believe the writer of the paper was sincere, but I think the opinion of the enlightened profession throughout the world is similar to my own. Nothing good comes from this thing, and the less that is said about it in medical bodies the better we are off. I do not know anything about hypnotism, and never expect to know anything about it, and certainly do not want to. I know that it has attracted the attention of some very reputable people, but Charcot, if I am not mistaken, before his death, practically reversed the statements that he had made during his lifetime. If he did not reverse them, he certainly revised them very materially, and, so far as I know, very little is being done with it by reputable people.

Dr. H. D. Lawhead, of Woodland: I think the writer of the paper, whom I know personally, has not written just as he feels on the subject. I think he may have been a little under the influence himself, and he has probably drifted off a little further than he was conscious of. I think the greatest mistake a physician can make is to give any credence to things of that sort. We had just as well proclaim that we believe in homeopathy in all its aspects, and that we believe in all the other "isms," hypnotism, mesmerism, etc. The physician is doing the dignity of his profession a wrong when he gives any sort of support to them.

The President: Hypnotism is not yet dead. It does not, however, hold the position it did have a few years ago. The way it was applied to everything certainly caused it to be classed with humbugs. I think, however, there is a field for hypnotism. With regard to Charcot, I attended his clinics in 1885—his private clinic—and saw him hypnotize people (usually hysterical people) and heard him lecture on the subject and express his views. He is opposed to the views of another school. Charcot was at the Salpetrière Hospital. Bernheim, of Nancy, is the advocate of the opposite school. He does apply it in certain cases of hysteria or functional neuroses with some benefit. I have had very little experience with hypnotism. I have tried hypnotizing the insane and had very little success. I will mention a case that I had a few days ago. It was a case of what some writers would call the insanity of fear. The patient, a young man, spoke to me in a soft whisper, so soft that I could hardly understand him unless I got very close to his mouth. In the course of the examination, which lasted three-quarters of an hour, I failed to get anything more than a whisper from him. He was sure he had lost his voice; persecutors had done this; he did not know who they were or what had caused it, but he had lost his voice, and he was never going to regain it. I first tried to see if force would do any good; I do not mean corporeal punishment, but urging him to talk loud, and I failed. So I turned to the attendant and said to him: "You will please bring this patient to the office this afternoon. and we will take him to the dark room and perform an operation on his larynx, and that will restore his voice." In the afternoon he was brought to the office. I had him taken to a dark room, had the light lit, and looked into his throat, and I said to him beforehand: "Now, sir, I am going to insert an instrument into your throat, and if you don't say 'Ah' when it is in there, it will go right down the larynx, and I might have some trouble in getting it out again." When I got the mirror in place he said "Ah," and, after I accomplished that, I was sure I would get him to talk; so I made him say all the vowels, and finally got him to say whole words while the mirror was in place, and before he left the room he talked loud. He had presence of mind enough to turn the result to his own favor, for, when he went into the ward again, he turned to me and said, in a loud, normal voice: "Doctor, now I am all right, why don't you let me go?" I just give this as an illustration of what might be called suggestion. I don't think, as I said this morning, that suggestion or hypnotism will ever amount to anything. Especially within the past year it has lost ground, and even among neuropathologists, where it has had its application, it is not used very much. As far as the insane are concerned, I do not think anything will ever be accomplished.

DR. A. T. HUDSON, of Stockton: Physicians and surgeons are in the habit of dealing with reason and common sense as the basis of all action. That is a pretty safe guide. Still, we are meeting things every day that our reason does not quite explain. That there is something in the mental forces there is no question. The old name of mesmerism or magnetism is a verity. It is a fact, and one that some can make a good deal of use of, others but little, and yet I do not think it has reached the position of a science whereby it can be taught sufficiently to be made any accurate use of in general practice. Many years ago a book was written by Dr. Esdale, who was an English officer in India. He gave the history of a large number of operations that he performed under mesmerism wholly. He took out a large number of scrotal tumors, and in each instance he used mesmerism alone. Patients would be under it for half an hour or an hour, and would come out without any unpleasant results. These are facts from which we cannot escape. I have myself seen some things done under mesmerism. I was about to operate once on a woman for fistula. Dr. Brown, who was assisting me, proceeded to give chloroform. The patient had taken, perhaps, three or four inspirations, when she began to talk. She said: "Yes, go on; she don't know it; she is all right; go ahead; proceed." I hardly knew what it meant, but I took my probe and put it in the fistula, and she kept on talking, until finally I followed the probe with my knife and laid the fistula open. She kept talking all the time: "Isn't that queer; I never saw anybody cure a hole by cutting it bigger," and such observations, just as though she was a third party; and all the chloroform that was given was perhaps a half-dozen inhalations. What that was I do not pretend to say.

The Removal of Foreign Bodies from the Superficial Tissues of the Eye, and the Treatment of the Resulting Lesions.—In the absence of the author, WM. ELLERY BRIGGS, of Sacramento, this paper was read by J. H. PARKINSON, of Sacramento. The author said: The frequency and importance of these accidents makes their proper treatment very urgent. Most foreign bodies in the eye can be easily removed, but occasionally the greatest possible skill is required to preserve the vision unimpaired. The bodies most commonly lodged in the eye are steel, rock, cinders, emery powder, sand, insects, grain, husks, lime, etc. If the foreign body comes directly from the fire, and is therefore sterile, it is much less likely to set up septic trouble than are pieces of rock, grain, husks, etc. Every eye presenting symptoms of irritation, not produced by a known cause, should be carefully examined with a strong convex lens, focal illumination, or with the ophthalmoscope, and if no foreign body is found, a drop of the following solution should be placed on the cornea: Fluorescin, gr. x; Sodii carb. gr. xvi; Aquæ distill, ℥i. If an abrasion is present, a greenish discoloration of the tissue takes place. When there is a posibility of as grain husk having entered the eye, the lid must be everted and the fornix examined. The injuries must be treated according to strict principles of antiseptic surgery. Instruments, dressings, and solutions, must be sterilized. In case of suppuration of the cornea, the galvano-cautery, or subconjunctival injections of bichloride must be made, unless treatment with antiseptics and hot water cause prompt improvement.

DR. J. H. PARKINSON, of Sacramento: There are one or two points that suggest themselves to me in connection with the removal of foreign bodies. One is the, perhaps, obvious fact that before attempting to remove it, you should be perfectly certain that the foreign body is there and that you see it. I have seen medical men attempt to remove something that they were not perfectly certain was there, and, as any damage to the superficial layers of the cornea, particularly toward the center line, may be followed by an opacity, this is a serious matter. A central opacity is a bar to vision, or interferes more or less with it. I have found that a very great assistance in discovering small foreign bodies, is what Dr. Briggs calls oblique illumination of the cornea with a lens. In this way you get a powerful light, and it is not confusing. The ophthalmoscope is something that few general practitioners can use. It is not such a difficult instrument to handle, but the majority of men are not familiar with its use. Cleanliness is also a very important thing. I believe a great many men think that because the eye is continually irrigated with the lachrymal secretion it is in fairly good condition. I think, on the whole, medical men are very careless in regard to aseptic or antiseptic precautions in ocular lesions. I think that is partly due to the fact that the eye is continually exposed, and there is an innate feeling that the lachrymal secretion will act as an irrigating fluid. It is not so very difficult to set up a dangerous and extensive inflammation, nor is panophthalmitis impossible. Any inflammatory action that is deeper than the superficial tissues, of course, is permanently injurious to the eye. Adhesions of any kind are permanently injurious. I think, perhaps, the most important thing in the paper is the recommendation of antiseptic precautions, and the avoidance of any manipulation of the eye, unless the operator be tolerably certain of what he is doing.

DR. T. W. HUNTINGTON, of Sacramento: I find in conversation with medical men that very few of them are familiar with one very simple method of removing foreign bodies that are lodged upon the under surface on the upper lid. It is very common for a person who is so afflicted to take the finger and rub heavily upon the lid. It is a fact that can be readily demonstrated that the simplest way of removing, for instance, a cinder from that locality is by simply placing one finger upon the upper lid and gently rubbing it from side to side. I have many times, while traveling upon railroad trains, run against people who were suffering from that cause, and have aided them in the removal of a cinder in that way without the slightest difficulty and with great relief. The mechanical movement seems to dislodge the foreign body, and it will, in a great percentage of cases at least, avoid the affair becoming firmly imbedded before assistance is reached. Where assistance is necessary, and where I personally have been called upon to remove bodies from that part of the human system by any amount of force, I have simply resorted to the plan of taking the corner of a silk handkerchief and everting the lid and picking the foreign body up on the corner of the silk handkerchief. That is preferable, I believe, where the object will yield to it, to any harder substance like an instrument.

DR. J. S. CAMERON, of Red Bluff: About 20 years ago a conductor on a railroad train taught me something about how to remove bodies from under the upper eyelid that I have found very efficacious. I got one under my own upper lid on the train, and I tried to get somebody on the train to evert the lid and wipe it off, and could not do it. The conductor came along and told me to take hold of the lashes of the upper lid and draw them down as far as I could and work the eyeball. Inside of five minutes I had it out.

DR. B. M. GILL, of Dunsmuir: Silk handkerchiefs are not always clean. Almost every day I have some foreign body to remove from the eye, and I keep some sterilized absorbent cotton in a receptacle for this purpose. When a man comes in I take a very small piece of that cotton and wrap it around a toothpick, and use that as my instrument to extract the foreign body. It is the best that I can find, and there is no possible chance of infecting the eye with anything.

THE PRESIDENT: I might relate a little experience I had some 8 years ago in regard to the removal of foreign bodies from the cornea. I was in an experimental mood, and the case was sent to me, there being no oculist in Stockton at that time, by Dr. Asa Clark. The man was working on the railroad, and a piece of steel flew into the eye and lodged at the rim or border of the cornea. It passed in diagonally, and the end of it was a little below the surface. No doubt it might have been removed by a knife, but I thought I would try an electro-magnet. I did not have one and resorted to the use of one that was used in a physical laboratory. It was a powerful magnet. I had a file prepared of proper thinness as a point and properly disinfected, and I had no trouble in removing the little splinter of steel.

Public Hygiene.—DR. J. R. LAINE, of Sacramento, read a paper upon this subject. The speaker traced the progress of hygiene from its earliest associations with religious observances since the time of Moses, whose excellent sanitary regulations were invested with a religious significance. In the early Greek history a further separation occurred. Medicine and hygiene were differentiated, though retaining their religious attributes. In modern times the two sciences—hygiene and medicine—were most intimately related, it being in many cases impossible for the surgeon or physician to properly conduct the management of his case without directions as to ventilation, clothing, water, etc. The relation of the physician to the public is increasing in importance as the population increases. He is brought in contact with legally constituted authorities, such as boards of health, registrars of vital statistics, health officers, and coroners. He is held responsible in a great measure for the position he takes and the advice he gives in relation to public health matters. It is true, whether his attitude on these questions be active or passive, his silence will mean that in his opinion no necessity exists for action or change. Of late the general public have awakened to the importance of sanitary work. The family physician is not infrequently asked for an opinion as to the healthfulness of a house or of a locality. The speaker then alluded in detail to various factors involving the contamination of air, water, and various foods which he said were more and more engaging the attention of the public. Public hygiene is founded upon the tabulation of causes of mortality, and the comparison of these statistics with those from other localities, and the gathering of vital statistics was also one of the labors devolving on state medicine. The efficiency of the public health service must depend largely upon the relations of its officials to the general medical profession. There must be confidence in the health officials from the profession regardless of any political or other consideration, or their efforts cannot reach the desired plane. In California the physician is compelled to report deaths and births on blanks supplied for that purpose, and a penalty is imposed of not less than $50 for violation of the statutes. Physicians do not hesitate to comply with the first, but openly violate the law with reference to reporting births. Many medical men evince a determined hostility to municipal regulations requiring notification of the existence of contagious and infectious diseases, holding that such should be made by the householder. The consideration of any such opposition to necessary rules and laws must be based on the idea that it is unjust for the State to exact services without remuneration. In those States where the registration of the physicians involves the granting of a license to practice, the State may demand a full report of each death from the medical man last in attendance without compensation. The State may demand more. The protection involved in his license against pretenders being in the nature of value received. Much greater difficulties are presented in the matter of compulsory notification of contagious and infectious diseases to the health office. The placing of a placard upon the house is done by the health authorities, but the notice is required of the physician making the diagnosis. Some physicians contend that such a requirement is in the nature of an exaction without compensation. The speaker held there were various reasons why these notices should be given by the person making the diagnosis, and asserted that compulsory notification by physicians must stand, even if in a measure unjust, and that the State is not asking too much when it requires a prompt notice of contagious and infectious diseases.

Dr. C. A. Ruggles, of Stockton: I endorse every sentiment expressed in the paper read by Dr. Lainé. It has been said that the practice of medicine is a high, holy and noble profession. I agree with that; but I also believe that those who are engaged in preventive medicine are engaged in a high, holy and noble profession. I can go back in my memory and remember when such a thing as preventive medicine hardly existed, and so can others in this room. The doctor was engaged in curing disease and not preventing it. The old woman would laugh at him, almost, when he was hunting for his bread and butter by curing disease, if he undertook to cut off the supply of that bread and butter. I might say that all the advancement in hygiene has been within the past twelve years. The use of the microscope has almost revolutionized hygiene and established positively the idea of preventive medicine. Why? Because it will show you the cause of disease. It has been my province to be connected with boards of health, State and local, for twenty-five years. I have seen the rise and progress of this hygienic theory, and I am glad to say that within fifteen years the progress has been so rapid that it has been almost miraculous.

Three years ago we had an epidemic of diphtheria in Stockton, and, when the quarantine and restrictive measures were put into operation, many people there wanted to run the health officer out of town. I did not go, though. To-day, let a case of diphtheria break out in Stockton and they will come and notify you before the physician does, of the existence of that disease, because they wish to be protected by the quarantine regulations which they know will be enforced. I agree with what has been mentioned about the absolute necessity of physicians reporting cases of contagious diseases. The little town in which I live, I am happy to say, has some regulations which other towns of larger pretensions would do well to follow. They require a physician there to report a case of diphtheria within five hours to the Health Officer, and if he does not do it there is a penalty thereunto attached. It is the same with smallpox, scarlatina and the like. Physicians should regard themselves as guardians of the public health, as preventers of disease, as well as curators of it.

Dr. J. H. Parkinson, of Sacramento: It seems to me that the contention that because the State affords a man a license to practice it thereby extends to him a protection against quacks, itinerants and charlatans, is hardly borne out by the facts. I did not understand Dr. Lainé to say that California did so; his remarks applied to those States that did. Certainly, in this State, no protection is afforded to the profession by the license issued by our Board of Examiners. That argument, to my mind, is similar to the argument made by lawyers from time to time, that because our profession is exempt from jury duty, it should be willing to give expert testimony for nothing. The reason the profession is exempt from the jury panel is not because of any regard for the profession, but simply and solely because the community wants to have the doctor handy at any and all times, and not because it wants to spare him anything. I have always contended that the notification of infectious diseases and births, and all these things, is imposing a burden on the profession that they are not justified in assuming. The only way that can be decided is by testing it. As long as it is the law, it is the duty of doctors, as good citizens, to obey the law. I do not advocate any disobedience of it, because it is the duty of the citizen to obey the law as he finds it. Nevertheless, that does not affect the principle. The fact of the amount of compensation involved does not cut any figure in the matter. It is the principle. Our profession at all times has been willing to do anything and everything for the public service. It has been diffident about demanding anything in return, and it gets very little. We get very little from the public except hard usage, and knocks, occasionally. We do not get any particular credit for many of the advances in sanitary science, and many things that we do are a direct injury to ourselves. The very unpleasant matter of expert testimony is particularly galling to all of us. We know the great injustice that is thereby inflicted on the profession. I think the reason death certificates are forthcoming promptly is that the profession has no trouble in regard to them; the undertaker brings a certificate to the doctor and the doctor fills it out, and the undertaker takes it away. As regards births, certainly the parent should attend to that. It is something in which the physician directly has no interest whatever, and in which the parent and future child are very much interested. The only interest the physician has is the indirect one of accumulation of vital statistics. There are cases in which no physician would be justified in neglecting to report a case of contagious disease. For instance, in lodging or tenement houses, or in places where the community would be directly exposed to danger. In private houses, where isolation is practically secured, and where ample means are at hand to provide for disinfection, and so on, there is less necessity. I say again, that I am not advocating disobedience to the law, or non-compliance with it, but I say, and I have good men on my side, that this assumption that it is the proper thing for us to do, is a mistake. Our rights are generally ignored, and we ought not to seek, from any unselfish motives, to put upon ourselves burdens that we are not justified in assuming.

Dr. Thos. Ross, of Sacramento: As regards the reports of births and deaths, and the requirements of the laws of the State, I think, as good citizens, we should obey them. It is not asking too much from us. As regards the reporting of contagious diseases, I think it is absolutely incumbent upon us as guardians of the public health, as we are, to report those, no matter whether they occur in a palace or in a hovel. I do not think there ever was a time when more attention was paid to sanitary science by the medical profession, than there is to-day. While the doctor was reading his paper, it occurred to me that some improvements might be made in our laws of prevention. I think it would be a good idea for our Boards of Education in cities and counties to require every applicant for a teacher's certificate to pass an examination on sanitary science. They would thus be able to recognize the symptoms of contagious diseases of childhood, and if cases of diphtheria occurred in the neighborhood, they could make regular examinations of the throats of the children in their class, and if they found any suspicious conditions, the child could be sent home with instructions to the parents to send for a physician. If that provision could be carried out, I have no doubt it would lessen the mortality of our children in a great degree, especially from contagious diseases. In other words, it would be a nice thing to make every school teacher a deputy sanitary inspector.

Dr. D. H. Lawhead, of Woodland: It seems to me that to authorize teachers to inspect the throats of pupils would be taken as tacit permission or license by a good many teachers, to go on and suggest medication, and perhaps the result of their medication and advice would more than counteract the benefits that might accrue to the suffering or threatened children. The laity are very prone to medicate, and to medicate quite profusely at times, and they might do more harm than good.

Dr. J. R. Laine, of Sacramento: I believe that if any physician in Sacramento would hesitate to report to the Health Officer a case of scarlatina, and by reason of his failure to report, the contagion spread around in several families, and he should publicly give expression to his views to the effect that the reason he did not report that contagious disease to the Health Officer was because the State or city did not pay him five cents a word, public sentiment would be of such a character that he would almost be required to pack his grip and leave. I want to know whether public sentiment or the physician would be right. Must we always demand an equivalent for everything we give? Do not we every day violate that code of ethics? Do not we every day do something for the impecunious? Every business, no matter what it may be, takes chances on not getting any pay. We cannot claim to be that disinterested class of men, laboring for the public good, who get up nights to save life, if we will not attempt to save life by warning the public properly, as we are directed to do by the law. I do not take the same ground with reference to the report of births. As I have said in the paper, that is another question. One may very consistently claim that the householder is fully as capable of reporting a birth as a physician. It requires no expert knowledge. But Dr. Parkinson will not contend that the householder is as capable of determining whether a patient has a contagious disease as the physician. He will acknowledge, if pressed down to the point, that a mendacious person may go and report a case of contagious disease as existing in a house and do an injury, if it is permitted to be done by any unauthorized person. It is indubitably the moral as well as the legal duty of the physician to report those cases.

The Reduction in the Fees of Medical Examiners for Life Insurance.— The resolutions on this subject, adopted by the Sacramento Society for Medical Improvement, were read by the Secretary. He said: The Society of Yuba and Sutter counties adopted the same resolution, pledging members that they would not examine for less than $5. A few weeks ago I received a certificate from a company in Connecticut—I cannot recall the name, but it was some company in Hartford—stating that they had reduced their fees all the way from $5 down to $1. Where the policy was less than $1,000, the fee for the examination was $1, which included a urinary analysis. I politely enclosed the schedule in an envelope and sent it back to the Medical Director, and told him if they wanted to get a doctor to do cheap work to get one. I would like to hear some expression from the Society in regard to this matter.

Dr. J. S. Cameron, of Red Bluff: We have already signed an agreement here among ourselves not to make an examination for less than $5 under any condition.

Dr. Thos. Ross, of Sacramento: This question was discussed by the Sacramento Society. I am in hearty sympathy with the preamble and resolutions as read by the Secretary.

Dr. H. D. Lawhead, of Woodland: This matter was presented to the Yolo County Medical Society, and the same preamble and resolutions were read.

There was a universal expression of approval of those resolutions, but for some reason or other there was no formal adoption of them. I will see to it that there is formal action taken adopting them.

Dr. A. P. Tarter, of Tehama: I would like to be enlightened on one point, and that is, whether we will be allowed to make examinations and exact an additional amount from the agent. I received the schedule of rates from the New York Life Insurance Company some time ago, and you all remember it was in the shape of a postal card which left blank spaces saying, "I will do so and so for such an amount." I took the card and said, "I will not do so and so for such an amount. In case you receive any application through me you will understand that I have received an additional amount from the agent." That is the way I have been working it since that time.

Dr. T. W. Huntington, of Sacramento: In regard to the question of the agent making the amount good, I think there is a grave question as to the propriety of making that exception. That is in a way conceding to the company. It may not be so in fact, but it will certainly appear so. The company does not care whether the agent puts up the extra $2 or not. What they want is to get the medical examination; to get the service. I am not personally interested in this matter, and am not the examiner for any company, but I think it would be ill advised to accept the service with any understanding other than that the company should go back to its old practice of paying $5.

Dr. J. R. Laine, of Sacramento: When the resolution was first introduced in the Sacramento Society I opposed it, and gave my reasons at that time, but did not have an opportunity of voting against it, which I should have done. I think it is in the right direction. If by a vote of this Society, or of all societies of the State, we can prevent the other societies from lowering the rate, as the two most powerful companies in the United States have done, we will have accomplished considerable; we will have saved something. But with the times as they have been throughout the United States we shall not be able to compel those two companies to recede from the position which they have taken, for this State will probably be the only State that will in any considerable degree "kick," so to speak. Those two powerful and rich companies will not recede from that position on account of the position which a few of the profession will take. If we attempt to make war on them in any way they will simply get other examiners from the homeopathic and eclectic branches of medicine, and they will do the work, if there is work enough in the place to do to make it an object to do so. In Sacramento, as well as other places, there are physicians belonging to the Medical Society continuing right along to examine, and they will do so in spite of any action of the Society. One man who voted for this resolution in Sacramento, went to another gentleman—the gentleman relates it to me, and I believe he told the truth—he said, "By the eternal, if one of those two companies wants me to do its examining, I will do it, even if I have to resign from the Society." The simple reason why he voted for it was because he was doing no examining. The man's necessity will force him to take that position. I do not believe in the movement. I do not believe we can force them, and it seems to me sometimes absurd to do things that you feel sure you cannot sustain.

Dr. J. H. Parkinson, of Sacramento: It is a fact that other States have taken this up. I know of Florida, Carolina, and Texas. A society in Carolina passed a resolution to this effect prior to any action by the Sacramento Society for Medical Improvement. The physicians in Red Bluff did the same thing before we took final action in the matter. I believe it is admitted that the Mutual Life is the strongest company to-day, therefore the other two companies are second and third. It is perfectly evident that if the two companies make it go the other companies will fall into line afterwards. Just at present some of the companies have sense enough to see that they have the best of the two companies that are trying to force this schedule, and that they are getting whatever good-will there may be from the profession, and that the other companies are getting a black eye. As to whether we can make it go or not, we certainly are not going to be any worse off by trying. It is perfectly certain that if the fees are reduced to this amount they will be reduced lower. Some few of us may lose something. I am one of them. I resigned long ago. I am willing to be out and injured individually.

The President: I am not an examiner for any life insurance company, but my sympathy has been with this movement. I think there is such a thing as fighting for principle, even if one should lose some money, and while it is possible that in the end this work will be worth nothing—the companies being victorious—still, there is some satisfaction in having done what is possible. Then, I believe it is possible that this will act as a boomerang; that it will hurt the companies to a considerable extent. I believe the life insurance companies to-day do not fully realize the importance of having thorough scientific work done by physicians.

DR. J. S. CAMERON, of Red Bluff: I begin to realize now how it is that I have got so many applications from the life insurance companies with reference to the qualifications of other physicians in Northern California. I believe it is these fellows who are making applications to undercut and take the places of the old examiners. I do not believe it is anything else. I think the next time I get an application from the New York Life Insurance Company—I have had three or four in the last three or four weeks—I intend to write and tell them there is not a man in Northern California who is capable of making an examination for it at the reduced figures.

DR. B. M. GILL, of Dunsmuir: About two years ago, I examined a man under protest. The agent said he thought he was a good man, that the company would take him. I protested, and told him I did not think he was. He was a neighbor of mine, a patron of mine; I had had many dollars from him. After making the examination, I was compelled to make an adverse report, and that man, to this day, thinks I have injured him. I have lost his practice, in a measure, and it has done me more harm than five or ten examinations would have done good. Examining for life insurance is not altogether an easy task. We are taking upon ourselves responsibilities sometimes that do not pay, and I believe that older practitioners who have plenty of work, can easily give up that business to younger ones, if the companies want them. If the companies feel as though they want to dispense with experience, they had better dispense with it than for us to undertake to do the work and take the responsibility for a small fee.

DR. ROSS, of Sacramento: On three occasions I had a similar experience to that of Dr. Gill, in losing families whose practice I had had for years and years, on account of being compelled to recommend the rejection of the applicant for insurance.

The preamble and resolutions were then put and carried unanimously.

Place of Next Meeting.—DR. E. E. STONE, of Marysville, on behalf of the Medical Society of Yuba and Sutter counties, extended a cordial invitation to hold the next meeting in Marysville The invitation was, on motion, accepted.

The Society then formally adjourned to meet in Marysville, on the second Tuesday in October.

New Members.—At the different sessions, the following were duly elected members of the Society: J. N. CRABB, A. J. LAPPEUS, J. A. OWEN, A. P. TARTER, H. J. WEST, and J, M. WEST.

SACRAMENTO SOCIETY FOR MEDICAL IMPROVEMENT.

Regular Meeting, February 18, 1896.

The President, C. B. NICHOLS, M.D., in the Chair.

Spontaneous Dislocation of the Shoulder Joint.—DR. J. H. PARKINSON reported a case of supposed rheumatism of the shoulder joint [patient exhibited], with the following history: J. S——, aged 69; farmer by occupation; came to my office on November 29, 1895, complaining of rheumatism. He was unable to move or to use his right arm. According to my note of the case, he stated that he had had rheumatism for four days, and that it had been getting worse, so much so that during the previous night he had been unable to rest. Any movement of the arm was quite painful, and, upon examination, I found that the joint was enlarged and swollen; being apparently twice as large as the sound side. The most painful spot was anteriorly, over the coracoid process, though neither this nor the acromion could be made out distinctly. I prescribed for him on the diagnosis of rheumatism, and on the following day he reported as being somewhat relieved. He came to my office on several occasions in December and less frequently in January. The improvement in the arm had been steady and uninterrupted. During all this time and until February 10th, patient had never been completely stripped for examination. During the first two or three weeks of his attendance at my office, his joint was too tender to permit of removal of his clothes without great difficulty and considerable pain. On the latter date, February 10th, upon stripping the patient, it was at once apparent that there was a posterior luxation of the shoulder joint, and his condition was practically as you see him to-night. At various times during the last two months, he has spoken of a "bunch" on his shoulder, but while I have had my hand on the joint on a number of occasions, I have never discovered the prominence that is now very evident. It is, unfortunately, impossible to obtain anything positive as to his past history. He says the joint has troubled him for seven or nine years; that four or five years ago he had very little power in that arm, and it will be noticed that the biceps of that side is very much smaller than on the left. On the other hand, there is no history of an injury, and the prompt subsidence of the acute symptoms in November, under salicylates, would seem to confirm the view that this was really a rheumatic attack.

Dr. W. A. Briggs: I saw this case yesterday, and after examining it and obtaining the history from Dr. Parkinson, I was inclined to lay a little more stress on the fact that the patient has been complaining for five or six years of pain in his shoulder, and that at intervals during that time he complained of having trouble with the joint, to such an extent as to incapacitate him for work. From this history it seems to me there has been a chronic joint affection which has filled up the cavity, in part, at least, so that some slight movement displaced the bone.

Dr. T. W. Huntington: There is every reason to substantiate the theory that has been put forward. There are no joints that may not be dislocated. The smaller joints are more frequently affected in this way, though, as has been noted by Dr. Bigelow, the hip joint may be displaced from chronic inflammatory exudation.

Dr. F. Gundrum: There is no doubt that this is a case of old standing. Some pathological condition has taken place in this joint years ago that has produced the present dislocation.

Dr. G. L. Simmons: The theory of chronicity is certainly the most favorable. In connection with the relief ascribed to the salicylates in this case, I remember an incident which occurred in an eastern city, when I was a young man. One night a friend of mine, who was the dispensary physician, was called out of bed to prescribe for a rheumatic patient, who was also suffering from diarrhea. He ordered a liniment and some chalk mixture, but the patient, by mistake, took some of the liniment internally and applied the chalk mixture locally. The symptoms due to this mistake were duly relieved, but my friend was surprised at the number of times the bottle of chalk mixture came back for repetition. Upon inquiry, he found that it was being extensively used as an external application in rheumatic cases.

The paper of the evening entitled "Chorea," was then read by **Dr. D. McLean**.

Dr. D'Arcy Power, in opening the discussion, said: The subject of chorea is an interesting one to me, because this disease is one of a group characterized by a certain pathology which has of late attracted much attention. I refer to rheumatism, gout, and neurasthenia, in all of which we have an excess of uric acid and urea. Chorea certainly bears a close relation to this group, and its association with it has long been observed. Statistics show that 25 per cent. of cases of chorea occur in rheumatic subjects. Seventy-five per cent. of cases of chorea occur between 10 and 15 years of age; of this proportion 24 per cent. are associated with rheumatism. This does not represent the direct relation between rheumatism and chorea, for if the chorea occurred later in life the proportion would be higher. Taking the symptomatology of the disease you will see that this relation goes further. A great proportion of the cases occurring in summer are associated with rheumatism. In chorea it is not uncommon to have heart lesions, and endocarditis is quite common in rheumatism. If chorea is one of the uric acid diseases we might believe that the line of treatment found efficacious in the uric acid diathesis would be serviceable here, and recent investigations have shown this to be the case. The use of arsenic in full doses, rest and controlling the night symptoms is the best treatment. I still think that the salicylates are useful in recurrent chorea, and one-third of these cases recur. These recurrences always take place in spring, similarly to rheumatism. I believe that if, when this period approaches, the salicylates were used, these symptoms might be averted. Regarding the question of rest, to keep the patient in bed is good practice, if it can be done without making him irritable, and you can very rarely do this with children. Non-stimulation of the emotions is good, as many of these cases occur from fright. Quite recently Hirt has been using eserine in doses of $\frac{1}{60}$ of a grain in cases where arsenic is not well borne.

DR. T. W. HUNTINGTON: Many children are afflicted in early life as a consequence of too little attention being paid to the morning meal. I frequently have had occasion to correct this evil in families where I have been called upon to prescribe for neurasthenic patients. It is a common practice in California to send a child to school upon a very light breakfast. In the cities, mush, with some very thin milk, constitutes the breakfast of a large number of school children. My advice is to send children to school upon a hearty breakfast with a liberal quantity of milk, and a considerable amount of animal food should also be given in the morning. I am surprised at the heroic use of arsenic by the author. I have used it largely, and regard it as a valuable remedy. I have, however, so often found grave symptoms following 10 drops of Fowler's solution that I have rarely carried it to that point. I should certainly feel that I was taking serious chances in administering 25 drops.

DR. J. H. PARKINSON: There is no doubt whatever as to the marked relation between chorea and rheumatism. In children with whom my experience has largely been, I have found arsenic almost as specific as quinine in malaria. Regarding the dosage, I have never given as high as has been mentioned to-night. I prescribe in minims, and after reaching a reasonable dosage have waited before increasing. I do not think I have ever given as high as 26, or even 20, minims three times a day. With small children judicious occupation is very desirable. It would be impossible to keep a small child in bed. Regarding the point raised by Dr. Huntington, I do not think it is so much the fault of the morning meal as the amount of food taken and the manner of its consumption. Children in a hurry to go to school will bolt their food and then rush off, which is certainly injurious. The diet of children up to 10 years of age should certainly consist very largely of milk and of the starchy foods, with meat in a lesser degree.

DR. W. A. BRIGGS: I understood the author of the evening's paper to treat the subject of acute chorea, and the discussion, it seems to me, has taken the same direction. Strictly speaking, acute chorea is probably the only chorea. Other diseases which have been classed with it having a different pathology. Chorea, acute chorea, I believe, is really an infectious disease. Several infectious diseases are associated with rheumatoid affections. For instance, scarlatina and gonorrhea, and the so-called rheumatism of chorea, I believe to be no more a cause of this disease than the rheumatoid affection of scarlatina is of scarlet fever itself. Rheumatism and chorea, like rheumatism and scarlet fever, or rheumatism and gonorrhea, have a common basis, and are parts of one and the same disease. I have obtained good results from small doses of Fowler's solution. Children bear arsenic better than adults. We can give children doses which an adult would hardly tolerate. The use of arsenic is the main element of treatment, with proper mental rest, because the disease spends its forces upon the nervous system. I have found very small doses of sulphonal to give good results, but large doses are unnecessary, according to my observation. Rest and the avoidance of excitement are important. Anemia is frequent in chorea, especially when associated with rheumatism. In these cases I have found iron very advantageous, particularly if associated with arsenic.

DR. F. GUNDRUM: The early writers considered that chorea had a direct relation to rheumatism. Later authorities do not take so much stock in this rheumatic idea; in fact, some of the recent foreign writers do not take any. Chorea is classed by some of our best men with hystero-epilepsy, and that is the view I take of it. There is just as much evidence that fright has some connection in producing chorea as rheumatism. I have used the arsenic treatment a number of times, but it is not the only treatment. I do not consider arsenic as much of a specific as some do, and I have never given it in such heroic doses. Antipyrin has recently been recommended to control the involuntary movements. The salts of zinc combined with iron are also useful. Winternitz is treating these cases to-day successfully and without medicine. I must take exception to the statement of Dr. Briggs, that the only chorea is the acute form. The authors that I have looked over say the acute form may become chronic.

Dr. H. L. Nichols: I have used arsenic, but not in such heroic doses. I increase the dose to 10 or 15, and sometimes 20 drops, three times a day, giving it with some iron preparation, the citrate being a favorite of mine. A sufficient amount of sleep is very important. I have found that bromide acts well. Chloral may also be used with good results. Properly regulated outdoor exercise is also very beneficial.

Dr. G. L. Simmons: In my earlier years I used to give arsenic, but I have abandoned it; finding that I get better effects from a general tonic treatment in connection with entire abstention from mental work, and, as Dr. Nichols says, all the good fresh air possible. None of the speakers have mentioned the use of quinine in this disease, in large doses, which has lately been discussed in the journals a good deal. Men like Weir Mitchell, and others as prominent in disorders of the nervous system, favor this plan. About six weeks ago a girl was brought to my office who, as Dr. Huntington has stated, had been breakfasting very lightly, and having a very irregular appetite, even after her return from school. The child was hardly able to walk into the office, owing to the irregular movements of the lower limbs. I put her on quinine, taking her books away, fed her regularly, and in ten days the movements had entirely ceased. I think this method of treatment is far ahead of arsenic or anything else that will tend to poison and debilitate the system. No reference has been made to the possible microbic origin of this disease.

Dr. Huntington: From time to time my attention has been called in Sacramento to a class of rheumatic patients. These cases suffer from muscular soreness or stiffness, with pain upon any movement of the body. I have treated them as though suffering from chronic malarial poisoning, using arsenic in considerable doses, and they readily yield to it. I mention these cases as they have some connection with the subject under discussion.

Dr. McLean, in replying, said: I would not like to commence the treatment of a case with 25 drops. I begin with 3 or 5, and increase until the higher dosage is reached, then stop and begin over again. Dr. Simmons' recommendation of the tonic line of treatment with iron and quinine is good, as the disease tends to get well itself. These cases are nearly always anemic, and iron will help them, but arsenic helps them more quickly. I have always found that children bear arsenic better than adults.

SAN FRANCISCO COUNTY MEDICAL SOCIETY.

Regular Meeting, February 11, 1896.

The President, Wm. Fitch Cheney, M.D., in the Chair.

New Members.—A. B. Grosse, M.D., and R. W. Payne, M.D., were duly elected members of the Society.

Dr. Emma S. Merritt read a paper on the "Differential Diagnosis of Diphtheria." [Published at page 179.]

Dr. D. W. Montgomery, in opening the discussion, said: The method of diagnosis referred to in this paper, is undoubtedly a good one, but it requires care, both on the part of those who prepare the tubes and with those who handle and inoculate them. Some may say that a negative reply by the bacteriologist would throw the clinician off his guard, but this objection could be raised against any method. The method is a good one to adopt for several reasons. It is well to know what part of the city contains the most diphtheria, and it is certainly good to know whether a case of membranous sore throat is true diphtheria. In this city, these tubes are now prepared by Dr. Spring, at the Polyclinic, and a diagnosis can, therefore, be arrived at just as surely as in New York. This fact should be known generally, by physicians, as it is the first time that the tubes have been available.

Dr. W. F. McNutt: No doubt it is time we should have better means for the positive diagnosis of diphtheria. At a meeting of the last Board of Health

I tried to persuade them to establish a laboratory and to provide tubes, and make examinations for physicians. This the Board would not do, the excuse being there was no money.

Dr. Albert Abrams: The pioneers in this method were Americans, of whom two, Drs. Biggs and Beebe, may be specially mentioned. Europeans were close followers. No reference has been made to-night to the pseudo-bacilli, which have, to a certain extent, the characteristics of the true bacilli, but are not virulent. Before we can certainly say that a bacillus is not virulent, we must have the animal as well as the laboratory experiment.

Dr. J. D. Arnold: We must all agree that any method, having as its aim the lessening of the terrors and epidemics of this dread disease, should be looked upon as a great stride in the right direction.

Dr. J. H. Barbat: Three years ago I read a paper based on the experiments of Park, of New York, who was the first to point out a distinct membranous sore throat not due to the true bacillus of diphtheria. At that time I exhibited the tubes, and since then have used them in every case of membranous sore throat. I certainly believe that the Board of Health should have these tubes ready at all times for physicians, and that when returned they should make the necessary report upon them. Dr. Park suggested that the swab be applied to the blood serum in the sterilized tube, and be then reinserted in its own sterilized tube.

Dr. C. G. Kuhlman: I am astonished to listen here to-night to the orthodox bacteriologists. There has been considerable discussion lately in the medical journals concerning bacteriologists. It seems strange to me that any physician whose nerve centers are well developed, can fall into the wake of these so-called "bug sharps." We can hardly accept the bacillus as a cause. Loeffler, himself, hesitated at this point, and did not come boldly forward until eight years ago. To-day he does not hold this view so strongly, for subsequent investigation seems to have thrown a doubt upon the matter. Many of the cases reported are of mixed infection. Out of 6,511 cases that were sent to the Board of Health, in New York, less than 2,000 proved to be diphtheria. As a result of these cases, that Board has abolished the tubes. In Berlin, according to the *Berlin med. Wochenschr.* of November and December, 1894, twenty of the leading physicians of that city are reported as stating that half the cases were not caused by the bacillus. Recently, in London, there was a discussion as to which was the virulent bacillus. Some believed it was the long one, others the short, and some the clubbed, and no conclusion was reached. Statistics here in San Francisco are of interest. For the year 1887 there were 35 deaths from diphtheria and 34 from croup. During 1889, when there seemed to be an epidemic over the whole world, the death rate here reached 105 from diphtheria and 77 from croup. In 1890 there were 312 deaths from diphtheria and 143 from croup. In 1891 the deaths had declined, before antitoxine had been used here.

Dr. Merritt, in replying, said: I did not mention the pseudo-bacilli, as the paper was a long one. It is news to me that the serum tubes have been abandoned by the Board of Health of New York, as they were in use there very lately.

Dr. W. F. McNutt read a paper on "Acute Bright's Disease."

Dr. M. Krotoszyner said: This case is an interesting one from several points of view. The author's explanation of the ascites is good. He has not, however, stated whether this was produced by an organic inflammation of the kidney. The amount of fluid withdrawn is often very great. In one case there was hydro-nephrosis causing the ascites. I tapped and repeatedly withdrew large amounts of fluid, and finally operated. As to large amounts of fluid in cavities, I may say that lately I withdrew two gallons from the left pleural cavity.

Dr. Albert Abrams thought, from the symptoms, that this case seemed to be due more to portal obstruction. The urine, with a specific gravity of 1010, and light in color, solidifying, nearly, by heat, indicated rather an exudation than a transudation.

Dr. C. G. Kulhman: I feel that Dr. McNutt was right; there would not be so much albumin if nephritis were not present. We do not always find elements in the urine to guide us. An acute condition might cause contraction of the kidney, preventing secretion. If an obstruction existed the fluid would be less in quantity.

Dr. McNutt: I may state, in reply to Dr. Abrams, that I have had the urine examined several times and found epithelia and granular casts. There is no question about this. It would be curious to find anasarca of the lower extremities and ascites, with albumin equaling 1½ per cent. and tube casts, in a case of portal obstruction. In some cases of heart disease we find mitral insufficiency with obstruction of the pulmonary circulation and right heart, and albumin in the urine. In enlargement of the liver, we find the urine of high color and high specific gravity. It is a very common thing to find porter-colored urine in portal obstruction; in this case there was no obstruction. When he first came to me there was ascites, and the first time his urine was examined casts were found. If he lives, he will have a cirrhotic kidney with a large amount of urine and a small amount of albumin.

Dr. Abrams: I am glad this case is now placed in the right light. My observations were based on Dr. McNutt's paper, as he read it, where no mention was made of the facts just stated.

Dr. H. B. A. Kugeler read a paper on "Epithelioma of the Esophagus" and exhibited a specimen.

The Kuhlman Resolutions.—The resolutions presented by Dr. Kuhlman at the last regular meeting were then taken up, and on a vote being taken, were lost.

The Reduction in the Fees of Medical Examiners for Life Insurance.—The resolutions adopted by the Sacramento Society for Medical Improvement [*vide* Medical Times, Vol. X, page 100,] regarding the reduction in the fees of medical examiners for life insurance, were then read and adopted.

Dr. W. F. McNutt said: To accept a reduction of this character would be a disgrace. The physician's responsibility in life insurance is very great. Other officers of the companies are paid handsomely. One concern pays its President $100,000, and one, whose President was no longer able to serve, retired him on a pension of $50,000 per annum.

The Secretary inquired if this action bound every member of the Society not to make an examination for any "old line" company for less than $5.

The President said: It most certainly did.

CALIFORNIA ACADEMY OF MEDICINE.

Regular Meeting, January 18, 1896.

The President, J. M. Williamson, M. D., in the Chair.

Extra Genital Chancre.—Dr. Dudley Tait reported and exhibited two cases of extra genital chancre. *Case 1* was that of a burn on the neck which had been treated for four or five weeks. At the site of this burn, an ulcer, about one and one-half inches in diameter, developed. It had indurated, elevated margins, and there was cervical adenitis and hypertrophy of the tonsils. The burn was curetted and skin-grafting was done by Wolff's and by Thiersch's methods. From the very first appearance of the ulcer, syphilis was suspected, and this view was confirmed by the rapidly increasing adenitis, and to-day there are additional evidences on the skin and scalp. *Case 2.* The young man from whom the grafts were taken. All the wounds healed well. Those made by Wolff's method were sutured and healed in five days, except one, which developed into an ulcer, almost the exact counterpart of that on the neck of Case 1. Some epitrochlear adenitis was developed. This ulcer appeared just three weeks and four days after the operation. The operation was done antiseptically. The infection in Case 2 undoubtedly occurred from the razor: It was aseptic at first, but evidently touched the chancre on the neck, when the graft was carried to it. The physician used three grafts.

Dr. D. W. Montgomery.—There is one good point in this last case: It has the value of an exact experiment, giving, as it does, the length of time between the inoculation and the appearance of the chancre. Almost all authors on this subject place the period at two weeks, which I think is too short. Many of the French works say three weeks, which is more probable. Certainly the period of incubation may be longer or shorter. We have several methods of incubation, other than by the regular route: as tattooers, who have mucous patches in their mouths, and who inoculate by needles that are moistened by the lips. An instance of several men in a barrack becoming inoculated through this process is on record. Under our care at the Polyclinic there is a young girl with a large ulcer on her lower lip. She appears salivated. Her gums are swollen; the sub-mental glands are indurated, and there is a roseola over the body. She gave a history of being the mistress of a man who gave her up, but she denied having anything to do with any one else. She lived in the same house with a girl who was an intimate friend of hers, and who had ulcers on her body. These two girls were inveterate cigarette smokers and often smoked the same cigarette: that is, one would lay one down and the other pick it up and take a puff or two. This girl had a crack in the lower lip near the median line, and the cigarette was, no doubt, the medium of infection.

Dr. D. D. Crowley: When did the chancre appear on the man?

Dr. Tait: His physician first saw it four days after the patient had noted it. Two weeks after the operation there was no sign of an ulcer. It was about the end of the third and middle of the fourth week that he noticed the ulcer formation, and at the end of the fourth and beginning of the fifth week epitrochlear adenitis made its appearance, and the rash is now about one week old. The speaker asked members what they thought of the extirpation of the chancre, not with a view of warding off systemic manifestations, but as a plastic operation. He also reported another case of gumma, in addition to those mentioned at the last meeting, and again called attention to the failure of iodide of potassium and bichloride of mercury in such conditions.

Dr. Montgomery: I always treat these cases locally with mercurial ointment; the results are good. This case of chancre of the lip was very painful and it was almost immediately relieved by local measures. As to the removal of the chancre for plastic purposes, it is not necessary. Take, for instance, this one of the lip: the ulcer is large, but it is at the expense of the chancroidal tissue, and the loss of real tissue is trifling; not nearly so great as one would expect. I cannot see the advantage of a cutting operation or of skin-grafting. In a case three weeks ago where the chancre was in the corner of the mouth, healing took place with only a slight depression at the site, and even that will eventually pass away. Apparently, there is going to be a large cicatrix. As to the treatment with iodide, we do run across cases where we seem to fill our patient with it, but without result. So, too, with mercury; many persons are refractory to it. Some cases withstand almost any treatment. A patient under my care now, with sores on his back, inside of thigh and on forearm, seemed to hold out against all mercurial treatments, although the ulcers got well for a time, first one, and then another. One case of tubercular syphilis of nose came to me on Christmas. It was exceedingly refractory to iodide, and it was well into the month of June before it could be brought under control of the iodide and mercurial ointment. Dr. Tait was right when he said that as a therapeutic test, the iodide of potassium was not an absolute one, for we do see cases where it has practically no action.

Dr. Crowley: In Oakland during the past 10 or 15 years I have treated many cases of syphilis. I have seen it in all its phases—chancres, eruptions, perforated palates, etc.; and I can say, truthfully, that in the primary and secondary conditions I have not seen a case where these remedies were not successful. There are anemic cases where we have to give iron at the same time as mercury. But mercury is as near an absolute specific as we can get. The protoiodide is my standby. I have seen wonders with from 1½ grains in a day. I have seen ulcers of the palate fade away in two weeks, and chancres in a week or ten days begin to close. I think that a judicious use of iodide and mercury will benefit any case. As to local applications, the oleates I consider the best. While using and giving mercury and iodide the patient should sweat profusely—best by Hammam baths.

Dr. Montgomery: We look upon mercury as a specific; and indeed syphilis, by its use, becomes one of the most controllable fevers that we have; no other exanthemata is so readily acted upon. I do not decry its value, most cases respond beautifully to it, and if persevered in most any case will be benefited by it some time or other.

Dr. W. Watt Kerr: In the great majority of cases, even where it would appear hopeless, improvement can be had, some only a very little, and some do fail. The protoiodide is good, and baths should be used. In one case nearly a year under treatment, but very little improvement took place. The mercury was dropped for five weeks and the patient put upon Bland's pills, after which the mercury acted far better, and from one to another the patient was finally greatly benefited. The remedies were not given combined, because the patient was approaching ptyalism, and because when they had been combined a short time they hurt the stomach.

Dr. Dudley Tait: I would like to say one word in reply to Dr. Crowley. Pathological anatomy has demonstrated conclusively that in tertiary syphilis the sclerotic changes are a distinct neoplasm; for instance, Fournier's rectal syphilis, which is not a true syphilis.

A Large Ovarian Cyst.—Dr. D. D. Crowley reported a case of ovarian tumor, and exhibited the cyst. The patient had general anasarca of the abdomen, the walls hanging down over the hips; she weighed 300 pounds. Pulse rapid; no increase of temperature. The tumor pressed up toward the chest, and was very large. The question in diagnosis was between ascites and ovarian cyst, and the latter was decided upon. The incision extended for 10 inches between the umbilicus and pubis, and on account of the anasarca the depth from skin to peritoneum was fully 6 inches. Upon getting down to the outer layer of peritoneum a trocar was inserted into the tumor and a thick substance resembling soft green soap came out. It ran so slowly that it would have taken days to empty it. The trocar was then removed, the edges caught and pulled up into the wound and the fluid came in torrents. Several cysts were found above the incision which were filled with a substance too thick to run through the trocar. The intestines were adherent to the tumor, and on trying to detach them the mesentery was torn down to the spine; the mesenteric artery was torn and was ligated *en masse;* a continued suture was taken in the mesentery and intestines. The pedicle of the tumor was too large to allow the clamps to be used, so it was ligated by two double ligatures and tumor sac removed. The contents had contaminated the abdominal cavity, which was now washed out with 4 quarts of sterilized water. When it came to closing the abdominal wound, it was found that there was no needle long enough to sew it, so an in-and-out stitch up through the skin was employed. The tumor was an immense one, the sac alone weighed 15 pounds, and with the contents 75 pounds, without taking into consideration that caught in the sponges. After operation, patient suffered from nausea, which was controlled by ice The temperature ranged from 99° F. to 103° F.; this latter was on the third day.

Dr. Tait: The use of water in the abdominal cavity is now concluded to be detrimental. It is never best to use a trocar on an ascitic abdomen.

Dr. J. F. McCone: I should like to ask about the pedicle. How was it tied, and of what was it composed? The tumor was certainly an immense one, and the contents of the cyst are peculiar. As to washing out the abdomen, most operators do so when the process is indicated, and without bad results.

Dr. F. K. Orella: Last year I passed in Europe. I was especially interested in gynecology, attending the clinics of Pozzi, and of Martin and others, in Berlin. I should like to ask as to the number of planes in which the abdomen was closed—that is, whether the peritoneum was sutured alone, and then the fascia and skin. As I understund only one plane was taken.

Dr. Crowley: The pedicle contained all to the right of the broad ligament, fallopian tube, arteries, etc. The suturing of this wound could not have been done otherwise, and this idea of several planes is all "bosh." As to the use of sterilized water in the abdominal cavity, I have used it for years in operations upon men, and in intestinal surgical experiments in dogs, of which I have done a great deal, with no bad results. In tapping the tumor some of the contents escaped, so I had to wash the cavity out.

SPECIAL CORRESPONDENCE.

PARIS.

[FROM OUR OWN CORRESPONDENT.]

Pasteur's Funeral.—The Pasteur Institute.—The Institute's Centenary.—Hospital News.

The 5th of November is an epoch-making date in the history of French science. Louis Pasteur was interred with more than regal pomp. The public funeral accorded to him by the state was a tribute to the man who had gained the foremost place in science. This tribute found an echo in the hearts of all classes and of all nations. Supremacy in science is not an inheritance, nor can it be handed down to unworthy scions of noble houses. It has truly been said of Pasteur "in science he was a monarch," and this by the force of his genius. "One of the finest moral antitheses of this century is the life and career of this child of the Vosges Mountains, whose life was an example of his disinterested modesty, while world-wide glory and renown gathered round his labors and his name." On that memorable 5th of October Pasteur was followed to his grave by all classes of the French nation, by representatives of every reigning crowned head, and by delegates from every learned society, and from other bodies. It is impossible to mention all the societies and syndicates whose delegates formed groups in the imposing funeral cortege of this man whose death is a world-wide loss. These groups amounted to 14, in which more than a score of associated bodies were represented The President of the Republic, the Grand Duke Constantine, of Russia, and Prince Nicolaus, of Greece, were present. At the Cathedral M. Poincaré, Minister of Public Instruction, delivered the funeral oration, which, unlike its congeners, deserves to be remembered. A tribune was erected in front of the catafalque, on which rested Pasteur's bier, covered with flowers. After the mass M. Poincaré ascended the tribune and in a masterly and touching manner delivered a brilliant resume of Pasteur's grand life. This scientist has been a benefit to all humanity. He has silenced forever the pert ignorami who, parrot-like, insolently inquired, What does science do? He showed again that science is practical in its benefits, and it is Pasteur amongst all scientists who has given to this truth the most irrefragable demonstration. An enumeration of the scientific labors of Pasteur would be too long for the space at my disposal. I shall glance only at his researches, which can be classified in four groups: Fermentation, the silkworm malady, charbon, and hydrophobia. These alone prove his right to the title of benefactor of the human race. M. Poincaré, while honoring the savant dwelt with emphasis on the great and good qualities of the citizen, the husband, the father, and the individual. These are known and appreciated by Pasteur's intimate friends, but M. Poincaré has done well by unveiling them to the public gaze. What is called "Pasteurian Science" will not lose by the fact that its founder was an example of superlative moral worth joined to unrivaled scientific power.

M. Duclaux, the new Director of the Pasteur Institute, is one of Pasteur's earliest pupils, and was indicated as his successor by the deceased scientist. With this purpose in view he had been for some time associated with Pasteur as a sub-director of the institute. M. Duclaux was born at Aurillac, in 1840; formerly a pupil of the École Normale; he became Pasteur's aid and co-worker in his silkworm researches. Subsequently he was a professor at the Clermont and Lynn Faculties, and afterwards he was appointed professor of chemistry at the Sorbonne, a chair he now holds. M. Duclaux is a member of the Paris Académie des Sciences. He has enriched scientific literature by works of the highest merit on capilarity, on milk fermentation, and on many other subjects. He was the founder of the *Annales de l'Institute Pasteur;* the monthly criticisms which appear in it are from his pen, and have great weight in the scientific world owing to the ability and strict impartiality of the writer. M. Rocet, the new sub-director, was born at Confolens, in 1853. He began his medical studies at Clermont, where he worked in M. Duclaux' Laboratory. He finished his medical studies at Paris, and became Assistant at the Paris Medical Faculty, and "Preparateur" in M. Duclaux' Laboratory at the Sorbonne, and finally in Pasteur's Laboratory. At the Institute rue Dutot he was Chief of Service. He and M. Chamberland were Pasteur's co-workers in the researches on cholera in fowls, on charbon, and on rabies. M. Roux' most recent researches are those on diphtheria and the diphtheritic toxine, which resulted in perfecting the sero-therapic treatment of croup and diphtheria, which has justly brought him much renown and glory. The Pasteur Institute was founded in 1884, and notwithstanding the death of Pasteur its organization will remain intact, and its aim will be identical with that which characterized it during the life-time of its founder. Pasteur's body was temporarily placed in the Cathedral of Nôtre Dame, but its final resting place will be the grounds of the Pasteur Institute, where a vault is being prepared to receive it. At Arboes, which is Pasteur's birthplace, the Municipal Council has decided to erect a statue to him, and the Municipal College is to be called the Pasteur College.

The centenary of the École Polytechnique and that of the École Normale was celebrated by balls and fetes in keeping with youthful spirits and rising ambitions. The centenary of the French Institute, on the other hand, was celebrated with due gravity, as becoming that august and venerable body. Strangers are surprised that this anniversary was so modest an affair, and that foreign universities and academies were not invited to send representatives. It is true that associate members and foreign corresponding members were invited, but in family gatherings all members of the family are included. Such invitations as were issued were of this character, and quite unconnected with international importance or amenity. The first ceremony on the programme of the centenary of the Institute was a religious service in the church of St. Germain des Prés, in memory of deceased members of the Institute, Monseigneur Perraud officiating. At 2 o'clock there was a reception at the Palace of the Institute to associate and corresponding members, foreign and French. This little fete was a quiet, family gathering. There were no speeches, but *en revanche* there was a lunch. On Thursday, October 24th, at 2 o'clock there was a public meeting at the Sorbonne; the Overture of Joseph, by De Muluil, the first musician ever elected a member of the Institute, was rendered, with other more modern selections; the Minister of Public Instruction delivered an address. A morning performance at the Comédie Francaise, and an evening reception on the same day at the Elyseé Palace, to members of the Institute by the President of the Republic, to whom foreign members were presented, and on the following day a visit to the Duc d'Aumale, at Chantilly, completed the festivities in commemoration of the hundredth anniversary of the French Institute, which has a glorious past, a glorious present, and which will have a glorious future.

Discontented students and a recalcitrant professor have been instrumental in again bringing before the public, medical questions. The basis of the discontent among the students is one of dollars and cents. The Assistance Publique has abolished the payment of twenty cents a day formerly accorded to hospital dressers, in hospitals some distance from the center of the city. The reason given for this action was, that distance is no longer an element of importance, owing to the great facilities for transportation. The students, not participating in this view of the case, prevented the competitive examination for hospital dressers from taking place, and smashed the furniture in the examination hall. They have held meetings, sent deputations to the Municipal Council and the Director of the Assistance Publique. The Dean of the Paris Medical Faculty achieved the herculean feat of calming these ebulitions during a meeting held at the faculty lecture theater, and was accepted as an intermediary between the students and the Director of the Assistance Publique. The latter subsequently sent a message to the students' meeting that the fees for the hospital dressers in outside hospitals would be restored. The students, in return, consented to the examinations being held, and they accordingly took place on the 28th of October. Professor Dieulafoy's opposition to the powers that be, proceeded from a more satisfactory reason, namely, an interest in his patients and his profession. The new regulation directs that the out-door hospital department be entrusted to a medical man, not attached to the hospital, but specially appointed by the Assistance Publique. Dr. Dieulafoy, who objects to the new regulation, is one of the Chiefs of Service who has regularly and conscientiously attended to the outdoor department of his wards, carefully examining and prescribing for the patients, and selecting the urgent cases for admission, and publicly announces his intention to continue this practice. On the outdoor consultation day a goodly number of students were present. Dr. Dieulafoy, with his body guard, appeared at the consulting room simultaneously with Dr. Méry, the Assistance Publique Special. The latter, with very good taste, withdrew, and Dr. Dieulafoy officiated, as in former times. The Hospital administration expressed its willingness to admit Dr. Dieulafoy's patients this time, but what it will do next time remains to be seen.

PARIS, February 5, 1896.

GERMANY.

[FROM OUR OWN CORRESPONDENT.]

The X-rays of Professor Roentgen.—Theory and Practical Application.—Reports of Cases.—The Advisability of Cycling.—Various Antitoxins.—Tuberculin in Veterinary Practice.—People's Homes for Consumptives.

Few scientific discoveries have created so intense a sensation, both in medical and in lay circles, as that of the so-called X-rays by Professor Roentgen, of Würzburg. No doubt your readers will, from other sources, ere now, be well acquainted with the theory and *technique* of this new kind of photography, but it seems only fair toward the discoverer, to recapitulate the first steps which led to this remarkable new departure, before plunging into the flood of practical experiments and improvements in detail, which will evidently soon push the original invention right into oblivion.

The history of the discovery dates back to the late Professor Hertz, of Bonn, whose observations on the connection between light and electricity aroused so much interest a short while ago. In pursuing further research on the same lines, his pupil and follower, Lenard, found that in the light emanating from Crooks-Hittorf tubes, the cathodal rays were absorbed by various substances in various degrees, and further, that the atmosphere completely absorbed them at a certain distance of some centimeters, according to the dilution of the air in the tube. He and his master had also observed that there are various kinds of cathodal rays, which are distinguishable by certain differences in their power of producing phosphorescence, their absorbability, and the degree of deflection by the magnet. Working with similar factors, Roentgen found that a certain effect of the fluorescent rays was appreciable at a much greater distance than Lenard stated, and that a sensitive photographic plate of barium-platino-cyanide was affected, even though protected by a thick layer of cardboard, wood, and other substances. As we now all know, these rays pass through the soft tissues of the animal body to a greater or lesser degree, being arrested only by the bones, the blood vessels, chemical deposits, and other denser bodies. Metals are less permeable by them than wood, and this latter again less so than leather, paper, woolen fabrics, etc. In consequence of this difference of permeability, it is possible to produce tracings of the outlines of various superimposed bodies (metal weights in a box, coins in a purse, and so forth), and this is the practical side of the matter. As to the exact nature of these rays, it is not yet possible to speak quite positively. That they are not merely a part of the cathodal light, is evident from the fact alone that they suffer no deflection by the magnet. Nor are they of the nature of the ultra-violet rays, for these latter are distinguishable from them by their properties of not being deflected by water, glass, zinc, etc.; by their resisting the ordinary polarizing agents, and by other specific qualities. The professor inclines to the assumption that their cardinal peculiarity is that they are rays vibrating in a longitudinal instead of a transverse direction, an opinion for which he advances several excellent reasons, so that, for the present at least, we may accept his arguments as probably correct. In all probability, they are produced by the cathodal rays in the walls of the glass tube, and can be formed under similar circumstances in other media, *e.g.*, in aluminium.

Be their exact nature what it may, clinical experience has already shown that this discovery is not only a startling revelation of things that were hitherto not dreamed of in our philosophy, but also a palpable advance for practical purposes. From all parts reports are pouring in of its successful application. In the Berlin Medical Society, Professor Koenig (lately removed hither from Goettingen, in place of the late Professor Bardeleben), showed an interesting case of sarcoma of the tibia, and expressed his conviction of the usefulness of the new method. A lady had suffered, two years ago, from pains and swelling of both knee-joints, but the symptoms had gradually diminished, leaving, however, a thickening of the upper extremity of the tibia and considerable weakness in the knee of the right side. On being recently consulted, Koenig, after repeated examination, diagnosed a neoplasm and advised amputation above the knee, which was thereupon carried out. The parts removed were, without further preparation, then skiographed (as Roentgen's method has been fitly termed), and the picture obtained plainly showed that the normal osseous substance terminated about at the upper limit of the epiphysis, while this latter was much lighter, being only traversed at parts by nebulous shadows, and bordered by a thin line of healthy dark (bone) shadow. The marrow was also, to a certain extent, invaded by this lighter substance. On incising the preparation, it was found that these portions exactly corresponded to a sacculated and acinous tumor which proved to be a sarcoma. Dr. Iastrowitz gave a detailed report on the subject, and exhibited a number of pictures. One of them showed plainly a splinter of glass imbedded in the hand of a laboring man, where it had remained for a number of years without having been detected, although the patient had difficulty in moving one of his fingers, and was positive that some glass must still be there, while other pieces had already been extracted. In one of the later meetings, Dr. Huber also exhibited three interesting skiographs: the first was a case of arthritis deformans of the finger-joints, with a history of previous gonorrhea. The thickening of the affected parts, and especially the hypertrophy of the bones was very conspicuous. Another case was an example of arthritis urica, also of the fingers. The articular ends of the bones not only presented marked thickening, but the deposits of uric acid were plainly visible, in the shape of a number of dark streaks. A comparison of the two cases will show that the diagnostic value of the method is not to be denied. The War Office has taken the matter up, and is having a series of skiographs prepared, in order to determine the value of the method in localizing missiles lodged in the human body during battle. No doubt some very interesting facts will shortly be forthcoming, especially as it appears from recent reports that it will be possible to obtain practically available tracings by much simpler methods than the somewhat costly apparatus hitherto employed. In Hamburg, it is reported, Professor Kiesling has succeeded in photographing a fetus *in utero.* I am unable to assert anything positive on this point, but if it proves to be true, we shall soon be all having pictures of our entire interior anatomy, and our hearts and kidneys will be thoroughly tested. The laity, in fact, have rushed far ahead of the profession in their conclusions as to the revolutions to be expected, on all sides, from the new art, as the following lines from the "up to date" poet, will show.

"I'm spell-bound with admiration,
  When the X-rays traverse thy inside;
Thy bones are bewitchingly lovely—
  My truly adored one, my bride!"

In comparison with such a phenomenal discovery all other recent medical achievements seem to fall rather flat. Still there have been some interesting and animated discussions in the Medical Society of Berlin lately. Dr. Mendelssohn broached the question of the healthiness or otherwise of cycling, a subject which has acquired some importance in our country, although the number of votaries of this sport is not so large here as with some other nations, and lady cyclists especially are comparatively rare. In general he expressed himself as greatly in favor of this kind of exercise, and doubtlessly there are thousands among the somewhat lethargic sons of the Fatherland who would derive the greatest benefit from it. Our gymnastics are excellent in their way, but are apt to become a little monotonous and dull. There is not enough excitement in them, whereas the bicycle combines amusement, fresh air, fresh scenes, and healthy exercise. Its chief dangers are over-exertion, especially in riding uphill. This is evinced by its effect on the heart, especially if that organ is not in a perfectly sound condition. A sad example of this was the late Prof. Haak, of Freiberg, who, as a medical man, might have been expected to use more caution. He had evidently been attempting the ascent of a somewhat stiff incline and had succumbed in the endeavor, being found dead on his tricycle some hours later. Hypertrophy of the heart, dilatation and nervous excitability have also been traced to this source. After sharp riding the frequency of the heart beats is often so enormously increased, as many as 250 in the minute having been placed on record. With due precaution, however, Dr. Mendelssohn summed up, cycling is an excellent thing and in this the members taking part in the discussion in general agreed with him.

The success of the antitoxin treatment of diphtheria seems to have set many minds at work to find methods on similar lines for treating various other diseases. Emmerich and Scholl, of Munich, have produced a serum from erysipelas with which they claim a cure for cancer. But their preparation does not seem to be of much value; several notable surgeons, among others Angerer, of Munich, and Czerny, of Heidelberg, having publicly expressed themselves as dissatisfied with the results obtained. Klemperer and Levy, of Strassburg, have produced an anti-typhoid serum from the dog by injecting a broth culture of the bacilli of typhoid fever in the usual manner. Their observations are not yet concluded, but they believe themselves justified in asserting that this juice is at least perfectly innocuous. Hitherto they have tried it on 5 patients, giving subcutaneous injections of 20 cubic centimeters on three consecutive days. All five cases defervesced in about a week after commencing treatment, but the authors leave the question of *post* or *propter hoc* undecided. Beumer and Peiper have experimented in a similar way with the serum of sheep. They have tested it on guinea pigs which had been artificially infected with typhoid virus, and the beneficial effect was evident. As yet they have not had an opportunity of applying it to human subjects, but in healthy persons it appeared to be devoid of injurious effects.

Evidently the tendency of modern therapeutics is to find an antitoxin for every disease. For this achievement we are *imprimis* indebted to Louis Pasteur's invaluable discoveries, and in part, let me add, to Koch's tuberculin, of which, however, at present it seems no mention may be made. Only in veterinary practice our lost pet appears to have found refuge, for reports from time to time crop up which go to prove that its diagnostic value is undeniably considerable in bovine tuberculosis. Dr. Eber, of Dresden, reports on a series of 563 applications. In more than 86 per cent. the diagnosis was fully confirmed by examination after slaughtering, and in most of the cases of mistaken diagnosis other diseases were found which would also have rendered speedy slaughter necessary. Only 26 animals had resisted the influence of the tuberculin, although they were actually suffering from tuberculosis, as proved by inspection after killing. The value of a speedy recognition of the disease is acknowledged on all sides, as the bacilli undoubtedly pass into the milk, and in this way may readily transmit the infection. Perhaps Koch's discovery will, in a modified form, prove after all of more value than we are now inclined to believe.

For the present the treatment of tuberculosis is referred in the first line to the aid of hygiene measures in all their forms. The Sanatoria of Goebersdorf and Falkenstein have had numberless successors, and now a lively movement is on foot to institute such homes for the poorer classes. Under royal patronage Prof. Leyden has long been busy in this direction. Bleichroeder, one of the first bankers of Berlin, gave a million marks ($250,000) for the purpose, and the whole matter, as regards this town, is at present only suffering from a plethora, a rivalry in philanthropy, as there are three or four humanitarian societies attempting to take the matter in hand, so that nobody knows which party is to be entrusted with the honorable duty. For the moment, meaning several months, perhaps years in this case, from a sheer superfluity of good intentions the poor patients are debarred from enjoying the benefits of which they stand greatly in need.

BERLIN, March 7, 1896.

CORRESPONDENCE.

Sir: Being examiner for nearly all the "old line" companies at this place, I am pleased to see that the physicians throughout the State are so unanimously in accord against the reduction of fees by these companies. Nearly everything has already been said that can be on this subject. I wish simply to say this to the insurance companies, that I have learned through hard experience that we cannot get something for nothing in this world. If the companies wish to have first-class examinations made, they must pay first-class prices therefor, and $3 per examination is not a first-class price. I am etc.,

Davisville, Cal. W. E BATES.

Sir: A united profession should strenuously resent the proposed reduction in fees for examinations in insurance, for the following reasons: (1) The medical profession should determine its own fees. Under no circumstances should it permit any corporation, however powerful or imperious, to dictate terms of service. This, it seems to me, is so evident as to require for its demonstration nothing more than mere statement. (2) The proposed fees are incommensurate with the service rendered and the responsibility incurred. Examinations in life insurance require exceptional training, character and judgment. They often make considerable demands on the physician's time, and often interfere with other and, to him, more important work. In case of rejection of an applicant, whether by the examiner or the home office, even the former fees were by no means an adequate compensation for the probable enmity of the applicant. The responsibility incurred may not be very accurately expressed in dollars and cents, but, if life insurance companies want examiners who neither feel nor regard such responsibility, they have only to proceed in their present course to have their want supplied. (3) Reduction of the fees by the two companies proposing it, means reduction all along the line. Rival companies, however unwilling to cut fees, will be compelled to do so in self-defense. What but the firm and united action of the profession is to hinder further reduction, as in various secret orders, even to the vanishing point?

I am, etc., WALLACE A. BRIGGS.

212 J Street, Sacramento.

The Reduction in Fees of Medical Examiners for Life Insurance.

Sir: The timely editorial comments of THE MEDICAL TIMES upon the action of two of the leading American life insurance companies, in reducing the fees of their medical examiners, commend themselves to the unqualified approval of every physician. Your able review of the subject has presented the matter in its true light, and has attracted wide attention. The arguments therein advanced are conclusive, and leave nothing to be added, except the assurance that the position you have taken is fully in accord with the views of your many readers. To submit to the reduction of a fee which, considering the character of the work required and the responsibility imposed, must be regarded as a minimum charge, would be to acknowledge that we place a very low estimate upon the value of our time, of professional opinion, and advice. The medical department of a life insurance company occupies a very important place in its management, and it has always been the policy of every company to maintain a close, confidential relation between the medical examiners and the home office. And the reason is obvious; for its stability, its very existence, in fact, depends upon the care and circumspection exercised in the selection of risks. The whole scheme of life insurance is based upon tables of life expectancy, compiled from the known average duration of a number of *healthy* lives. Hence, every acceptable applicant must be of sound bodily health; "free from any tendency to disease," acquired or inherited; of faultless habits, unobjectionable family history, etc.; and all this must be ascertained by, certified to, and incorporated in the report of the medical examiner—surely a somewhat arduous task. Some months ago, in replying to a communication from the medical director of the New York Life, notifying me of the proposed reduction, I ventured to suggest that, in view of the work involved in the preparation of such a report, and of the vital importance to the company of the accuracy of the information furnished, a fee of $5, or of even double that amount, could not be considered a reckless expenditure of the company's funds; that the directors of his company, upon reflection, could not fail to appreciate the absurdity of a "graded fee" for the performance of the same identical work; and that, in my opinion, if such a "fee" were forced upon them physicians would, in keeping with the dignity of their position, insist that it be upon an *ascending* instead of a *descending* scale. Gratuitous services, unless the object be charity, are seldom appreciated, and to accept from a wealthy corporation a smaller fee than is usually paid for similar services by individuals, in even moderate circumstances, would provoke, and justly subject us to well deserved ridicule. The prompt action of the Sacramento Society for Medical Improvement, was a dignified stand for reasonable compensation for a service rendered. Let physicians in every community, in like manner, make known their determination to resist any humiliating attempt on the part of wealthy corporate bodies to depreciate the value of medical services conscientiously performed, and these and other life insurance companies will soon be forced to the conclusion that their efforts at retrenchment would better be undertaken along other lines. I am, etc., DAVID POWELL.

Marysville, Cal.

REVIEWS AND NOTICES.

THE FUNCTIONAL EXAMINATION OF THE EYE. By J. Herbert Claiborne, M.D., Adjunct Professor of Ophthalmology in the New York Polyclinic; Instructor in Ophthalmology College of Physicians and Surgeons, New York, etc. With 21 illustrations. Philadelphia: The Edwards & Docker Co. pp. 100. Price, $1.00.

The author of this little work says it is his purpose to make the subject clear beyond peradventure, and to present it in such a way that a student may follow the lines laid down, and perform the examinations with scientific and mechanical accuracy, without further instruction. In part of his task the author has succeeded well. The subject is presented in a clear; precise, and simple manner, and is, therefore, admirable for students. It will repay any one for the time required in looking through its pages, but it is not, in our opinion, broad enough for a complete guide to the functional examination of the eyes.

COLOR-VISION AND COLOR-BLINDNESS. A PRACTICAL MANUAL FOR RAILROAD SURGEONS. By J. Ellis Jennings, M.D. (Univ. Penna.), formerly Clinical Assistant Royal London Ophthalmic Hospital (Moorfields); Lecturer on Ophthalmoscopy and Chief of the Eye Clinic in the Beaumont Hospital Medical College; Ophthalmic and Aural Surgeon to the St. Louis Mullanphy and Methodist Deaconess Hospitals; Consulting Oculist to the Missouri, Kansas, and Texas Railway System. Illustrated with one colored full-page plate and 21 photo-engravings. Philadelphia: The F. A. Davis Co. pp. 110. Price, cloth, $1.00.

This little book of 115 pages contains the principles of practical importance to those examining for color-blindness. Although it does not go into the subject deeply from a theoretical standpoint, the author has accomplished what he desired to produce, "a practical work on color-blindness, which shall contain all that is essential to a perfect understanding of the subject, and to refer the reader to the proper authorities for many of the facts stated."

A TEXT-BOOK UPON THE PATHOGENIC BACTERIA, FOR STUDENTS OF MEDICINE AND PHYSICIANS. By Joseph McFarland, M.D., Demonstrator of Pathological History and Lecturer on Bacteriology in the Medical Department of the University of Pennsylvania; Pathologist to the Rush Hospital for Consumption and Allied Diseases. With 113 illustrations. Philadelphia: W. B. Saunders. pp. 360. Price, $2.50.

This book is chiefly characterized by perspicuity of diction and terseness of expression. It is not a decided addition to the surfeit of works on this subject; true, in consequence of its recent publication, many new affections have been introduced, which are naturally disregarded in older works on bacteriology. The technical procedures are decidedly incomplete. Thus, if one were to seek information relative to the incubator and thermo-regulator, important appurtenances in bacteriological work, he would not find it in this book. This work does not embrace the entire subject of parisitology, but is limited in the main to a study of pathogenic bacteria. Many of the fungi are likewise omitted. Many of the photographic reproductions, like those in works on the same subject, are very indefinite.

PREGNANCY, LABOR, AND THE PUERPERAL STATE. By Egbert H. Grandin, M.D., Consulting Surgeon to the New York Maternity Hospital; Consulting Gynecologist to the French Hospital, N. Y., etc.; and George W. Jarman, M.D., Obstetric Surgeon to the New York Maternity Hospital; Gynecologist to the Cancer Hospital, N. Y., etc. Illustrated with 41 original full-page photographic plates from nature. Philadelphia: The F. A. Davis Company. pp. viii–261. Price, cloth, $2.50.

This is a companion book to the authors' previous work on obstetric surgery, the present volume being intended to form, with the other, a compend on obstetrics. The book is eminently practical, occupied but little with theories, and concisely and authoritatively presenting facts. It is not so much new facts, either, that are brought out, as it is the clear presentation of the old ones that recommends the book. The illustrations are especially valuable. They are photographed from life, and decidedly realistic, conveying as good an idea of the steps of labor, for instance, as actual observation by a student could give. Altogether, the book is well worth possessing, and should be read by every man who does obstetric work.

NEW TRUTHS IN OPHTHALMOLOGY, as developed by G. C. Savage, M.D., Professor of Ophthalmology in the Medical Department of the Vanderbilt University. Third edition, with fifty-eight illustrations. Nashville: Published by the author. pp. 270. Price $2.

The third edition of this work comes to us much enlarged over the second, which was published three years ago. The important part of the work is that devoted to the author's views upon the action of the oblique muscles in astigmatism, which have been extensively discussed in the journals and special societies during the past few years. Whether one believes the author to be a "fadist" or not, every ophthalmologist should study the question from an impartial point of view. Other questions discussed in this work are: Relationship between centers of accommodation and convergence; rhythmic exercise the proper method of developing the ocular muscles; sthenic and asthenic orthophoria, and sthenic and asthenic heterophoria; can presbyopia be deferred by rhythmic exercise of the ciliary muscles? The law of projection and the artificial and natural causes that modify it; the monoscopter. Besides these subjects, the author discusses heterophoria and its treatment; the necessity of mydriatics in testing for glasses, and several operations which he thinks improvements on the usual methods.

PRACTICAL DIETETICS, WITH SPECIAL REFERENCE TO DIET IN DISEASE. By W. Gilman Thompson, M.D., Professor of Materia Medica, Therapeutics, and Clinical Medicine, in the University of the City of New York; Visiting Physician to the Presbyterian and Bellevue Hospitals. Illustrated. New York: D. Appleton & Co. pp. 800. Price, cloth, $5; sheep, $6.

It is with the greatest pleasure that we welcome this volume. The necessity for a scientific and at the same time thoroughly practical work on dietetics, has long been felt; for, although very much has been written on the subject, at intervals, during the last few years, yet it generally has been for the purpose of "booming" some special food, rather than with the object of placing the matter on a scientific basis. It would be hard to imagine anything better adapted to its purpose, than the volume just issued by Appleton & Co. Not only is the arrangement of the different topics excellent, but the information furnished is just of the nature that is desired by every student. The earlier sections are occupied by a full description of the properties of the various food-stuffs, beverages, etc., and these are followed by instructions in the preparation of food, and by an account of the diets adapted to different diseases, with an explanation of the reasons why each diet is recommended. There are few books which will be found to be so useful to the practitioner of medicine as Dr. Thompson's volume on practical dietetics.

THE EVOLUTION OF THE DISEASES OF WOMEN. By W. Balls-Headley, M.A., M.D. (Cantab.), F. R. C. P. (Lond.), Lecturer on Midwifery and the Diseases of Women at the University of Melbourne. London: Smith, Elder & Co. pp. 376. Price, $4.00.

This book makes exceedingly interesting reading, and contains much original thought not to be found in any of the usual text-books on gynecology. It is not intended as a guide in practice, for but little space is devoted to the subject of treatment. Yet, in the chapter devoted to treatment, as in all other parts of the book, the originality of the author is refreshing. He is evidently deeply in earnest, and does not hesitate to call things by their right names. Speaking of the relation of the sexes on the evolution of the diseases of women, he says: "In woman the sexual instinct—that is, the natural, unreasoning impulse by which she is guided to the propagation of the race, is usually more pronounced; in man, the sexual appetite—that is, the desire of gratification in the act of union." Condemning the common treatment of lacerated cervix by local applications, he says: "If the glans penis and urethra of a man were by some accident so split that the raw faces of the wound rubbed at every moment against his legs or clothes, would it be thought wise to send him to bed for 6 or 12 months to avoid this friction; or to apply the actual cautery or caustics at frequent dates to a surface which can but rarely skin over, because the friction rubs off the advancing epithelial cells as fast as they are formed?" The book, as a whole, is not as practical as some others, but the reader who takes it up will not regret the time spent.

MEDICAL NEWS.

LICENTIATES OF THE BOARD OF EXAMINERS.

At a meeting of the Board of Examiners, held February 19, 1896, the following were granted certificates to practise medicine in this State:

Henry Joseph Ap John, Campo; Med. Dept. Willamette Univ., Or., April 4,'92.
Willis R. Cogdon, Santa Cruz; Rush Med. Coll., Ill., Feb. 19,'89.
O. H. Dove, Oakland; Med. Coll. of Indiana, Mar. 30,'93.
Arthur M. Gerow, Los Angeles; Med. Dept. Univ. of Buffalo, N. Y., Feb. 25,'68.
A. J. Gray, San Diego; Dartmouth Med. Coll., N. H., Oct. 31,'60.
H. P. Hargrave, Oakland; Univ. of Manitoba, Canada, April 17,'95.
J. C. Joralemon, Los Angeles; Rush Med. Coll., Ill., Feb. 21,'77.
H. Kohlmoos, San Francisco; Univ. of Giessen, Germany, Nov. 22,'93.
E. C. Lalonde, Los Angeles; School of Med. and Surg., Canada, May 10,'81.
Marshal Petitt, Los Angeles: Med. Coll. of Indiana, Feb. 28,'79.
Chas. S. Porter, Los Angeles; Med. Dept. Univ. City of N. Y., April 7, 93.
Philip K. Strong, Los Angeles; Bellevue Hosp. Med. Coll., N Y , Mar. 12,'88.
C. E. Vaughan, Santa Barbara; Harvard Med. Coll., Mass., July 15,'63
Gideon A. Weed, Berkelev; Rush Med. Coll., Ill., Feb. 2,'70.
Edith V. Anderson, San Francisco; Cooper Med. Coll , Cal., Dec. 6;'94.
Allen M. Carpenter, Berkeley; Cooper Med. Coll., Cal., Dec. 5,'95.
Clara May Holmes, Oakland; Cooper Med. Coll., Cal., Dec. 5.'95.
Birger Peters, San Francisco; Cooper Med. Coll., Cal., Dec. 5,'95.
Wm. W. Roblee, Riverside; Cooper Med. Coll., Cal., Dec. 5,'95.
Bertram Stone, Redding; Cooper Med. Coll , Cal., Dec. 5,'95; Med. Dept. Univ Cal., July 13,'95

CHAS. C. WADSWORTH, Secretary.

Official List of Changes in the Stations and Duties of Officers serving in the Medical Department of the U. S. Army (Division of the Pacific) from February 20, 1896, to March 20, 1896.

Captain William Stephenson, Assistant Surgeon, is relieved from further duty at Fort Canby., Wash., and will return to his proper station. S. O. 28, Dept. of the Columbia Feb. 18, 1896.

Leave of absence for ten days, to take effect on or about the 8th proximo, is granted Lieutenant Colonel Johnson V. D. Middleton, Deputy Surgeon General. Par. 4, S. O. 25, Dept. of Cal. Feb. 28, 1896.

Captain William Stephenson, Assistant Surgeon, will proceed at once to Fort Canby, Wash., and report to the commanding officer for temporary duty during the illness of Assistant Surgeon Benjamin Brooks Par. 1, S. O. 36, Dept. of the Columbia, March 5, 1896.

Upon the recommendation of the Chief Surgeon of the Department, First Lieutenant Paul F. Straub, Assistant Surgeon, will report to the commanding officer, Presidio of San Francisco, California, for temporary duty at that post until the arrival thereat of Major Robert H. White, Surgeon. Par. 2, S. O. 29, Dept. of Cal., March 9, 1896.

The following named officer of the Medical Department has been relieved from duty in Washington, to take effect upon the completion of his present course of instruction at the Army Medical School, and assigned to duty at the following station: First Lieutenant Irving W. Rand, Assistant Surgeon, Fort Apache, Arizona.

Official List of Changes of Stations and Duties of Medical Officers of the U. S. Marine Hospital Service (District of the Pacific) from January 20 to February 20, 1896.

Past Assistant Surgeon C. T. Peckham, to proceed from San Francisco Quarantine to Port Townsend, Washington, and assume command of Service.

Past Assistant Surgeon J. O. Cobb, when relieved at Port Townsend, Washington, to proceed to Cincinnati, Ohio, and assume command of Service.

Past Assistant Surgeon, M. J. Roseman, to assume command of San Francisco Quarantine Station.

ITEMS.

Dr. Anthony Huffaker has removed from San Leandro, Cal , to Carson, Nevada.

Dr. M. L. Petitt has removed his office from 1354 Howard street, San Francisco, to Cloverdale, California.

Dr. E. J. Boyes, of Oakland, will soon leave for an extended trip in the east and in Europe. The Doctor intends to be absent for six or eight months, and proposes to devote most of his attention to diseases of women and children in Philadelphia and New York, as well as abroad.